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A FUNCTIONAL STUDY OF OBSERVATION

By JOSEPH McVICKER HUNT, Cornell University

It is commonly said that the method of the natural sciences rests finally upon observation. To derive the facts of nature men observe. They observe with pains and care, trying to give a faithful account of what they see and hear. This kind of observation, which requires aptitude and training, is frequently called 'scientific observation.' But observing, taken in a broader sense, is an operation of the human organism which goes well beyond the tasks of the sciences. To dwell upon objects or events, noting their differences and their resemblances, classing and ordering them and detecting their changes and activities, is to observe. This observing kind of operation is, then, of frequent exercise. It is employed on many occasions and directed toward many ends.

In all the descriptive psychologies observing has been neglected. While the good observer has been extolled for his unusual powers, the very process of observing has itself never been adequately dealt with. It is, then, the purpose of the present study to describe the simpler and more elementary aspects of this common and important mode of activity.

In the history of psychology the accomplishments or the products of observation have been considered in various contexts, but generally under the heading of perception, of judgment, or of thinking. The setting has usually depended upon the systematic view of the investigator. For instance, when these three categories were united under the term *intellect,* and intellect was defined as a form of knowledge originating in 'sensation.' Attempts were then made to determine and to describe the laws by which sensory materials were combined in knowledge.[1] When judgment and thinking were considered as the higher mental functions or constellations, psychologists sought to ascertain the particular 'elements' or 'processes' involved and to determine their

* Accepted for publication August 1, 1933. This study was carried out in the Psychological Laboratories of Cornell University under the direction of Professor Madison Bentley.

[1] Alexander Bain, *The Senses and the Intellect,* II, 1855, 315-608.

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plans of arrangement.[2] While working from this standpoint men were forced to recognize certain functional aspects of 'mind.' They found it impossible to exhaust perceiving, judging, and thinking in terms of process. The observer's task or *Aufgabe* was found functionally determining the report.[3] Following the introduction of the concept of the task into experimental psychology, there was a long series of studies devoted to ascertaining its effects upon the reports given by observers in psychophysical and other experiments.[4] The attempt has also been made to bring the accomplishments of observation under configurational principles.[5] Most perceptual products are here termed 'configurative wholes.' 'Insight' appears, and these 'wholes' are described in terms of 'dynamical relationships,' including factors in the arrangement of the stimulus-pattern, the principle of 'closure,' the 'goal,' and the 'organic set.' Thus, although the accomplishments of observation have long been under consideration from various points of view, the observational performance itself has been generally neglected.

The purpose of our study is to obtain a functional description of simple observational performance. We shall not consider observation merely as a response to stimuli, as does Woodworth when he makes the term include the attending and perceptive responses of the organism to stimuli.[6] Nor shall we confuse the accomplishment or product of observation with the functional performance, as Wheeler does when he speaks of observation in one sentence as a kind of "intelligent behavior," and in another defines it as "the awareness of an object as a whole."[7] We shall attempt, by means of the method of report, to describe the human organism in the act of observing, to ascertain the varieties of mode in observational performance, to determine how the performances are initiated and governed, and to discover products and issues.

It appears from empirical examinations of observational performance

[2] This was the traditional procedure under the structural view of mind as represented by E. B. Titchener, *Textbook of Psychology*, 1910, and O. Külpe, *Grundriss der Psychologie*, 1893. For representative researches see E. Jacobson, On meaning and understanding, this JOURNAL, 22, 1911, 553-577; W. H. Pyle, An experimental study of expectation, *ibid.*, 20, 1909, 530 ff.; H. M. Clarke, Conscious attitudes, *ibid.*, 22, 1911, 214-249; S. W. Fernberger, An introspective analysis of the process of comparing, *Psychol. Monog.*, 26, 1919 (no. 117), 1-161; and S. C. Fisher, The process of generalizing abstraction, and its product, the general concept, *ibid.*, 21, 1916, (no. 90), 1-213.

[3] We refer particularly to the work of the Würzburg School. Cf. E. B. Titchener, *Experimental Psychology of the Thought-Processes*, 1909, 1-318.

[4] The following studies are representative: S. W. Fernberger, The effect of the attitude of the subject upon the measure of sensitivity, this JOURNAL, 25, 1914, 538-543; S. S. George, Attitude and psychophysical judgment, *ibid.*, 28, 1917, 1-37; C. L. Friedline, The discrimination of cutaneous patterns below the two-point limen, *ibid.*, 29, 1918, 400-419; H. Friedländer, Die Wahrnehmung der Schwere, *Zsch. f. Psychol.*, 83, 1920, 129-210; C. C. Pratt, Some qualitative aspects of bitonal complexes, this JOURNAL, 32, 1921, 490-515; A. C. Reid, The effect of varied instruction on the perception of lifted weights, *ibid.*, 35, 1924, 53-74.

[5] W. Köhler, *Gestalt Psychology*, Chaps. v, vi, and x, 1929, 1-394.

[6] R. S. Woodworth, *Psychology*, 1921, 365-425.

[7] R. H. Wheeler, *The Science of Psychology*, 1929, 137 ff.

and from the experimental investigations of the products and the antecedents of this performance that 'observation' includes more than one typical and outstanding mode of operation. As commonly employed, the term includes all modes of function from the simplest and barest perceiving to comprehension.[8]

Between these typical, outstanding modes of operation empirical evidence indicates that there stands at least one other mode, a mode involved in the scrutiny of objects and occurrences. *Inspecting* has been suggested as an appropriate name for this function.[9] While ascertaining the modal varieties in observational performance, one of our main tasks in this study will be to determine where inspecting enters and how it stands related to perceiving and to comprehending.

Our experiments will fall into three groups. In the first, Part I, we attempt to obtain by pictorial presentations a descriptive account of an entire observational performance, in order to determine the respective rôles played in it by perceiving and by inspecting. In the second, Part II, the experiments are devised to describe inspecting more adequately and to determine its exact functional relations. In the third, Part III, the investigation of observational activity is approached by way of certain of the psychophysical problems and methods, where the inspective mode seems to be explicitly involved.

Part I. Modes of Observational Performance

In this preliminary part of our study we sought a bird's-eye-view of all the variations of observational activity which are incited by the use of simple materials presented under a simple task. Performances with comparatively long temporal courses seemed desirable. From the observers' reports we hoped to determine whether the performance could be fractionated into plural modes.

Apparatus and setting. Sixteen pictorial presentations, cartoons and scenes without script (Fig. 1), were presented to 10 *O*s for an unrestricted time in a simple exposure apparatus.[10] *O* concluded each performance by pressing a key devised to drop the shutter. The instructions were as follows.

You will be presented with a series of pictures. Some of these pictures have significance beyond the mere scene portrayed. When the significance is found, im-

[8] For a discussion of perception as a functional mode, see M. Bentley, *The New Field of Psychology*, 1934, 18 ff., 32 ff., 119 ff.

[9] Bentley, *op. cit.*, 21, 266-274.

[10] The *O*s were Dr. Jenkins (*Je*), Dr. Feldman (*Fn*), Dr. MacLeod (*Mac*), of the instructional staff, Dr. Glanville (*Gl*) and Dr. Schubert (*Sch*), Mr. Roff (*Ro*), assistant in the department, Miss Stewart (*St*), and Mr. Neff (*Ne*), graduate students, Mr. Console (*Co*) an undergraduate, and Esther Hunt (*Eh*), the writer's wife.

mediately press the key to drop the shutter. If no significance can be found, press the key as soon as you are satisfied. After the exposure, report what you find and give, as completely as possible, the course of your performance.

The course of the performance and the functional modes involved. Though activity is continuous, each stage merging almost imperceptibly into the next, there is a definable course. Since the pictures were chosen to provide several gradations between no topical significance and a rich and obvious topical significance, each *O* took the task of formulating a

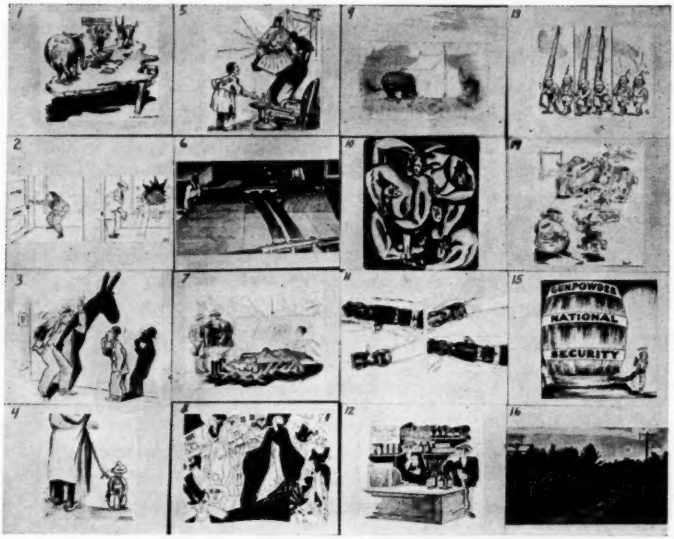

FIG. 1. STIMULUS-OBJECTS USED IN PART I

topic for every presentation. The time of exposure varied from 5 to 60 sec. Topics were actually formulated in only about a third of the performances, but those cases in which no topic appeared are more informing than those in which the topic came easily, telescoping and making the report difficult.[11]

In the composite performance there are three typical stages, each of which is characterized by a discrete mode of activity.

(1) First stage. There is an opening stage in which bare objects are immediately apprehended. The product here is the only part of the performance reportable, with

[11] For a discussion of the topic and its relation to comprehending see Bentley, *op. cit.*, 21, 266, 274-287, 400-405.

the occasional exception of characteristic strains which accompany search.[12] There the searching appears to have been initiated by the task taken with the 'ready' signal. Here are reports.

Co: The first thing I saw was the huge sergeant standing in the opening of the tent and a young man sitting up in bed. (7)[13]
Fn: First apprehended two individuals and the two shadows without relating them. (3)
Eb: The first thing seen was the form of a donkey or an ass and two men. (3)
Gl: The first thing noticed was the large man with a huge grimace on his face. Then I saw the figure of a man standing at the left. (5)
Je: First I saw the small figure of the individual on the right side, perceived as a boy. (4)
Mac: First I saw three pictures of two soldiers. I saw them as soldiers carrying guns before any significance flashed upon me. (13)
Ne: I didn't see anything clearly at first. Then I saw the bear and the general contours of the tent. (9)
Ro: Got the general impression of a cartoon from the first look. (1)
Sch: Almost instantly after the shutter fell I saw a big woman, and then noticed the small figure of a man. (4)

Occasionally these first impressions are accompanied or immediately followed by comments. These comments usually contain some evaluation of the first perceptual significance.

Co: The picture immediately suggested to me the work of X or of some one who draws for the New Yorker. (8)
Fn: When I first saw the picture, I commented, "Fairly complicated." (6)
Gl: When I first saw the picture, I thought, "this is one of those dizzy pictures I don't like." (8)

At times, however, these comments contain the beginnings of topical significance, as in the following report.

Eb: My first reaction was distaste. I am opposed to armaments, and when I saw guns in the picture, I was immediately opposed. (13)

In this instance the guns appear to have immediately represented armaments, a topic. This report is representative of performances in which the topic comes easily and soon. It also represents the foreshortening of the activity which may occur when *O* is prepared for the topic depicted.

It should be noted that there is no evidence of self-direction in the reports from this first stage. *O* is governed in his performance by factors in the occasion (occasional instruction), as is indicated by the fact that the first elements seen in the presentations are in some way outstanding, for their size, for their sharpness of outline, for their position, or for their novelty.[14] In the reports of most of the performances there is also evidence of the control exerted by the initial task to find significance. The following report emphasizes the influence of both of these governing factors.

St: First I saw the great twisted part, then my eyes were drawn hither and thither. One object appeared and then another, until I discovered the picture was composed of various parts of human beings. (10)

[12] For a description of search and its relation to the other psychological functions, see Bentley, *op. cit.*, pp. 20 f., 211, 215, 224, 238, 241 ff., 256, 294 ff., 314, 323, 329, 332, 364, 411.
[13] The number following each report refers to the serial order of presentation.
[14] For a description of the three forms of instruction (formal, self, and occasional), see Bentley, *op. cit.*, 29, 194 ff., 365-370.

There is no decision here to look at one part and then another. The task has initiated the performance; first one object and then another attracts *O*'s gaze.

(2) Second stage. The second stage typically involves a scrutiny of the objects apprehended in the first stage. *O* inspects the elements in the presentation to relate them and to discover any symbolic significance which they may have. Here are reports from this stage of performance.

Co: After a first impression, I had to look for a while before I could grasp any significance. I saw Uncle Sam sitting on the bench and the donkey representing the democratic party talking to him. Out in the field, I saw the G.O.P. elephant trying to make a touchdown in a football game. I looked back and forth trying to see the meaning. (14)
Fn: I compared the boy in the first picture to the one in the second to make sure they were the same. (2) At one time it occurred to me to look for something outside the picture to account for the shadow of the donkey. (3) Was puzzled at not finding a support for the model. Turned to the bridge on the right again, looking for a table-edge or something. Could find nothing. Finally I decided that it didn't matter anyway. (6)
Eh: I was aware of trying to see whether I knew that name, and whether, knowing the name, the picture would then have significance. (6)
Gl: There was then self-instruction to scrutinize again to find something which would give a clue to the meaning. I tried to find a symbolic meaning in the two figures. (3) I decided to find out what the mess was about. I was surprised to find it was a football game, and commented to myself that probably the Republican elephant was bungling. But when I examined the game, I found that the elephant really wasn't bungling. (14) At various times during the observation, there were flashes of meaning, "I should be getting along faster with this." (5)
Je: Then I began to scrutinize the figures, and I saw the tie going from the small individual's waist to the large individual. That was accompanied by the verbalized image, "He's tied to someone's apron strings." Then I wondered, "Whom does the big figure on the left represent? Why is he tied to her apron strings? What does Texas have to do with it anyway?" All this coming in flashes. (4)
Mac: I tried very hard to find some order in that chaos of legs and bodies. (10) I decided to look around for some element that might enlighten me further. (16)
Ne: I set myself the task of finding out to what these heads belonged. (10) I attempted to make an evaluation of the scene, since I have often observed such scenes in the mountains of Pennsylvania. (6)
Ro: At first they had no significance. I scrutinized them very carefully and saw the man coming out of the door. I discovered no oddity in his appearance, and so there was no apparent reason why the student should jump. (2)
Sch: The next thing that occurred to me was that they represented either a group of contortionists or a group of wrestlers. I scrutinized them to find out which. The large muscles pointed to their being wrestlers. Yet their position would just as well indicate contortionists. (10)
St: Supposing that the picture on the right was the explanation of the one on the left, I shifted my eyes to recognize a train. In examining them I noticed that the auto and train were approaching each other at such an angle that their paths would cross. I glanced to the right to see what had happened. It then occurred to me that the right picture might not be a separate one. I saw I was wrong. (11)

In this part of the performance the searching appears to be more intense than in the former stage. There are shifts from one part of the presentation to another, but here they are initiated by *O*'s fresh queries which continually renew the search. These are self-governed bits of observational performance.

The second stage of the performance appears distinctly different from the first. It consists in an effort to abstract from objects and events already apprehended aspects which will supply the answers to queries concerning the relationships between those objects. Frequently these attempts at abstraction are accompanied by flashes and verbalizations of comment which carry the relationships between or the symbolic significance of the objects already apprehended. In this way, these comments are different from those appearing occasionally in the first stage which comments were usually evaluative. The issues of this stage of the observational activity, thus, may be described as the relations between objects and their symbolic significance. The activity in this stage may be described as a fusion of search and perception in an attempt to solve a specific query.

The duration of this second stage is widely varied. When *O* is well prepared for the topic represented through interest or store of information, or when the topical significance is obvious, it may not appear at all. On the other hand, if *O* is not prepared or if the topic is obscure or even absent, he may fill a whole minute with this active scrutiny. A topic may never be formed.

Before the typical third stage is described, we must mention occasional instances in which imagination and memory are brought into play. In these *O* attempts to imagine situations which lend significance to the objects in the presentation, or he recalls articles he has read or incidents of the past which may contribute to his task. Here are just a few reports.

Gl: Then I imagined it was a railroad model stretched out on a table. I found, however, that this was not so. (6)
Je: There was then a peculiar series of flashes which were not at all solutions, but abortive starts at solution. "Is the man holding hope for both parties?" "Is he making them both back off the map?" (1)
Ne: It suddenly occurred to me that this represented my idea of Yellowstone Park. I have never seen the place, but this picture seemed to me to be as it should look. (9)
Eb: Looking at the picture further, I noticed a small bridge in the center, and that immediately brought back images of the crossings on Michigan Avenue in Chicago. With these in view, I knew what the picture represented. (6)

O forms something closely related to a topic from these memorial and imaginational activities. They are then discarded or verified by further inspection of the picture.

(3) Third stage. In the third typical stage the issues of the earlier observational activity are unified into a topic. *O* apprehends the objects, ascertains their relationships and looks for any symbolic significance they may have, then, with varying degrees of effort, he synthesizes them, formulating the significance of the whole presentation, the topic.

Co: These things did not have much significance as a whole, but perhaps the picture represents night life in a big city. (8)

Fn: Then the solution came at once, "Election fight." Thereafter, I saw the significance of the posture of the three figures, of the board, and of the map. The precise significance of these parts came in this time after the topic. (1)
Eb: Looking back at the railroad-figure, I saw evidences of something wrong. The word finances gave me the clue. I then came to the conclusion that the whole thing meant that Mr. Coolidge and his 'Rail Body' were going to help the railroads out of their financial difficulty. (5)
Gl: Then I wondered what all this meant, and then suddenly I said, "Why, the 'Coolidge Rail Body,' by its measures, expects to solve the financial ills of the railroads." (15)
Mac: Then I looked further down and saw the words "National Security." Immediately the significance of the cartoon flashed upon me. I saw the national security as a menace to peace. (15)
Ro: Then it all came to me, "Texas tied to the apron strings of her governess, Ma Ferguson." (4)
Sb: Suddenly I realized that this had reference to the election of Ma Ferguson as governor of Texas. (4)
St: Then I got the impression of "politics," and then proceeded to examine each part of the picture, deriving as much significance as possible, intending later to put them together and obtain the whole meaning. (1)

The last report (*St's*) illustrates well the course of the procedure. The performance consists in a sudden condensation of the products of the earlier part, the relationships between the objects apprehended and the bits of symbolic significance, and a formation of a whole, the topic.

Let us consider a few total performances. They reveal *O* performing continuously, working now in one mode now in another, but always directed toward the goal set by his task. They should obviate any impression that the stages are discrete. In the first the topic comes soon, and is enriched by further observations.

Eb: I looked, saw the picture, and the significance came to me almost instantly. It represents the state of Texas tied to Ma Ferguson's apron strings, indicating that she is to be their next governor. Further scrutiny yielded the significance that Ma is a force in Texas, that she cannot be moved but must be endured. (4)

The next ones are more typical, including all three stages in order.

Gl: I looked and saw three pairs of figures carrying guns. I said, "Ah, little boys playing soldier." Then I noticed that the faces of those carrying guns were mature. I gave myself a suggestion to begin at the left of the picture and proceed to the right. Then I found the two figures labelled "France" and "Germany." In noticing the expressions, I saw the unpleasant expressions of the left pictures changing to geniality in those on the right. Then I saw that the left pair were carrying heavy guns, and at the right they were carrying guns much smaller. Immediately the significance flashed out, "competitive armaments." The artist was showing that competitive armaments are burdensome, and that if France and Germany would both reduce their armaments, they would both be happier. (13)
Mac: As the shutter dropped I saw the blurred image of a large elephant. I recognized the scene almost at once as a representation of a football game. Then I looked down toward the bottom and saw the picture of an insignificant donkey, representing the Democrats, asking the coach to send him into the game in place of the very large elephant. The significance came in a flash. I saw the incongruity of the little donkey asking to take the place of a very large elephant; but the statement made by the cartoon seemd to be very unauthentic. (14)
Scb: At first I saw only the Republican elephant and the Democratic donkey. Then I saw the man representing Uncle Sam, but I had to lean forward to read the words, "Presidential Straw Vote." I continued to scrutinize the picture, but it took me a long time before I could make any sense at all of it. I couldn't see

why the elephant and the donkey should be so juxtaposed and what the man in the middle was doing, nor why the map of the U.S. should be raised. I struggled within myself to find out what the whole thing might mean. Then, very suddenly, it came to me that this was supposed to represent a balance, and that the straw vote was showing the equality of the balance between the two parties. (1)

These three typical stages of performance coincide surprisingly well with what we expected to find from empirical grounds and from former experimental investigations which were directed only toward the products

TABLE I

SCHEMA OF TYPICAL STAGES OF PERFORMANCE

	Stage I	Stage II		Stage III
Functional mode	Perceiving	Inspecting	Occasional reports of imaginational and memorial activity	Comprehending
Nature of performance	Automatic, little reportable besides products	Strained, a combination of searching and perceiving to solve a query		Automatic or strained, involves condensation and unification of issues from Stages I and II into 'topic.'
Antecedents	Task and exposure of presentation	Task, queries, and self-instructions		Task, queries, and accumulation of issues from performances of stages I and II
Governed by	Task and factors in the occasion	Task and by *O*'s self-instructions		Task and by issues of performances in stages I and II
Issues or products	Vague impressions, objects, and events, obvious relations, and some symbolic significance	Obscure relationships, symbolic significance, and aspects of objects answering *O*'s queries may lead to formation of topic or enrich it after its formation		The topic

and the antecedents of the functions. The first stage is typical perceptive activity. The second is distinct, and aptly termed inspection. The final stage, the third, is understanding, in which a topic is formulated from the products derived by perceiving and inspecting. The following schema (Table I) summarizes this part of our study.

PART II. DISTINGUISHING CHARACTERISTICS OF PERCEIVING AND INSPECTING

The purposes of the second part of our study were: (1) to obtain samples of observational activity at varying levels of complexity in order to refine the distinction between perceiving and inspecting and to test the results and interpretations of Part I, (2) to determine whether and how their antecedents differ, and (3) to determine whether each form of activity

has its own kind of product. The plan was to limit strictly the duration of the performances and to vary the materials presented and the task so that we might have reports of activity varying from bare apprehending to complicated inspecting from which to answer the proposed questions.

Apparatus and materials. The stimulus-objects (outline-drawings and linear extents) were pasted on pieces of card-board 125 x 45 mm. These were presented in a tachistoscope of the mirror-filter type. The time of the exposures was limited to approximately 100σ for Series 1–4 and for 6, and to 250σ for Series 5.

The tachistoscope was illuminated by a two-chamber light box. A current of 110 v. flowed through a 75-w. Mazda 'daylight' bulb. The lights for fixation and for exposure were actuated by means of a condenser-charge timer described elsewhere.[15] The experimental room had a low diffuse illumination.

With a stimulus-object in place in the exposing chamber, a 'ready-now' signal was given for fixation and preparation. After approximately 2 sec., *E* pressed a key to charge the condenser and so to light the exposing chamber. When the exposure had disappeared, *O* gave his report directly to a dictaphone.

Before the experiment began, a preliminary series of 10 outline-drawings was presented to familiarize *O* with the situation and to provide some training. As *O* dictated his reports, *E* judiciously questioned him in order to clarify his terminology and to round out the report.[16]

Series 1. Observation of single objects. With the presentation of the simple outline-drawings of familiar objects like those shown in Fig. 2 bare perceptual activity was expected. We sought this for comparison with more complicated forms to be obtained from later settings. The simplicity and familiarity of the objects would, as we thought, allow for immediate and automatic identification. The only observational task implied by the instructions was that the objects be identified.[17] The instructions were:

> A familiar object will appear for a very brief time. Observe and report exactly what you see. The report should include a complete account of your performance from the signal 'now' to the disappearance of the object.

A few more than half of the 280 reports, obtained from two presentations of the 20 objects in this series, describe the bare and simple form of observational activity expected.

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[15] J. G. Jenkins and W. D. Moeder, A condenser-charge timer, this JOURNAL, 45, 1933, 742-744.

[16] Dr. Feldman (*Fn*), Dr. Jenkins (*Je*), and Dr. MacLeod (*Mac*) of the instructional staff, Dr. Glanville (*Gl*), Mr. Roff (*Ro*), assistant in the department, Miss Bestor (*Be*), graduate student, and Mr. Console (*Co*), undergraduate, served regularly in these experiments. Dr. Fox (*Fx*), instructor in psychology at the University of Rochester, and the writer (*Hu*) observed in part of them.

[17] Here we must point out what would appear to be obvious, although—so far as we know—no one has ever been explicit about it. The verbal instruction given *O* in the beginning of a psychological experiment often has little or nothing to do with the initiation of the particular performance under examination. In these instructions only the first two sentences are relevant at all. The rest is necessary merely for experimental routine.

Co: This time I saw a shovel. That's all. (8)
Fn: The object was a shoe with high uppers. Instantaneous perception with comment "shoe." (15)
Gl: I was relatively passive until the object appeared, then I recognized it immediately as a cat. (4)
Je: This was a highly condensed apprehension of a single stage. It was a fish with head to the left, and accompanying the apprehension was a verbalized flash "fish." (14)
Mac: I am now set to see familiar objects. What appeared was an umbrella tipped somewhat to the left. I saw it immediately as an umbrella and my picture of it did not subsequently change. (3)
Ro: Here I had just adjusted my fixation when the object flashed out. It was simply the outline of a cow, appearing and then disappearing. (8)

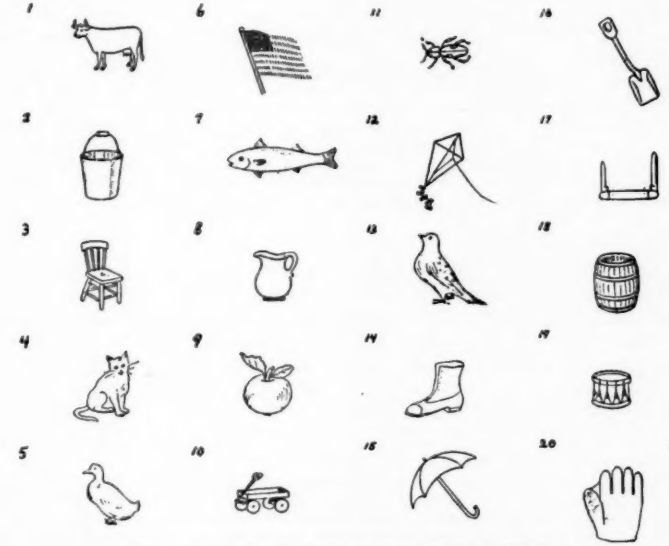

FIG. 2. STIMULUS-OBJECTS USED IN PART II, SERIES 1

In the last reports, identification is complete and immediately satisfactory. But the remainder of the reports describe activity much richer and more complex than was expected. When identification was not immediate and automatic, *O* increased his effort and the complexity of his activity. Every available resource he employed to satisfy that implicit task to identify. Here are reports describing the principal variations in the activity initiated in this setting.

(1) Delay. In some experiments identification is merely delayed. There is no scrutiny, just waiting.

Fn: Slight jerk of the body at 'now,' then perception of something disturbed by apprehension of movement in the field. Perturbed feeling, then just before disappearance came "straight" simultaneously with label "chair." (9)
Fx: First I saw something I called fuzzy or cottony, then there was movement in the field and the object took on form and became very definitely a cat. (4)
Mac: First it was a geometrical design, then, almost immediately afterwards, I saw the thing as a kite. The apprehension of the thing as a kite came during the period of the memorial after-image. (12)

(2) Substitution. One identification becomes unsatisfactory when *O* notes more details and he makes another. Such performances are not frequent. They are neither typical perceiving, nor, since they involve no self-governing, are they typical of the inspecting described in Part I, although they resemble the latter very much.

Be: A hand appeared. Was immediately attracted by a peculiar something between the fore-finger and the thumb. Looked like a web. Thought to myself the hand was not in position to hold such an object, so was rather nonplussed as to what it was. (20)
Fn: The first perception of the object was accompanied by the word "pyramid." That meant 'color pyramid.' Then the string stood out. I continued to fixate that part after the exposure had terminated. Finally came the comment "kite." (12)
Je: This was complex. First I apprehended a vague kinesthetic pull down toward the left upper corner of the exposure-field. This passed into a very direct lead to a vague and largely kinesthetic apprehension of a triangular shaped something toward the left side of the design. Then my eyes seemed to swing along an upper line, coming from the left side of the picture, and, just as the exposure ended, I apprehended the object as a small boy's wagon with the handle projecting upward and toward the right. (10)

(3) Supplementation. There are also, though not often, the typical inspective performances like those of the second stage in Part I. These appear when, for any reason, *O* fails to identify automatically in perceiving. The determination to identify in these instances is supplemented with a query concerning identity. In this *O* seeks particulars and details contributing to the identification. At times the activity is continued after the exposure has ended. Thus, inspecting may be initiated even by an implicit task to identify simple outline drawings.

Be: Something appeared with no meaning. Looked at it carefully to determine what it was, but it was gone before I could tell. (11)
Mac: This time I first simply perceived something there, a complicated something I could not recognize. Shortly after the close of the exposition, while examining the after-image, it occurred to me it might be a spider. (11)
Je: Complex. First, I apprehended a curved something. That soon passed over into a stage in which the exposed object was relatively unimportant and the felt object very important, typified by a flash, "Why, I don't know what that is." Accompanied by unpleasantness. Just before exposure was completed, I fixated upon the linear part in the center of the design. There was something I can describe as a pull towards the center and up. Then, suddenly there was a condensed verbalizing, "It's an apple." (9)

(4) Identification transcended. Finally, there are performances in which *O* goes beyond identification. The proportion of this kind of activity varies greatly for individual *O*s. The presence or absence of performances of this type, or the direction they take, is directly related to *O*'s preparation, *i.e.* *O*'s self-instructions govern his performance.[18] There are several variations.

[18] The influence of self-instruction in the psychological experiment, or elsewhere, cannot be too strongly emphasized. *O*'s own self-preparation is a most potent factor in determining the direction of his activity in any setting. This is, however, not a

(a) No additional preparation. Some *O*s are satisfied to identify objects.

Co: I think now that no matter how long the time might be between the ready signal and the exposure, there will be no strain, because the objects are very familiar, and I find them easy to report. This time I saw a shovel. (16)

Fn: Nothing in the foreperiod. A cat. (4)

(b) Others seek details for their own sake.

Be: Decided to look for details and particularly for anything which might be a little bit odd. A fish appeared. Noticed that there was something peculiar; the eyes seemed to be a little further back than is normal. (14) An umbrella appeared. Noticed the handle was turned and that the part protruding above the main seemed broken. (15)

(c) Observer *Gl* approached many of the exposures with several self-imposed tasks. As a result many of his performances were exceedingly complicated and active, although identifying alone is requested.

Gl: After the 'now' I set myself to apprehend very carefully. I was wondering whether the object would be familiar or just an arrangement of lines. I also wondered whether it would be one which I had seen before. As soon as the object appeared I said, "Ah, it is a cow, a new object." Just as it was disappearing I tried to determine what was the slight irregularity down near the hind leg. (1)

Series 2. Observation of geometrical objects. The purpose here was to complicate the performances by presenting geometrical figures which are fairly difficult to identify and by instructing *O* occasionally, through the sequence of figures, to go beyond apprehension to the inspection of relationships within and among them. There were eight discrete patterns besides those shown in Fig. 3, 2 at the beginning and 6 at the end of a series of 20. Between these were organized sequences of three, in each of which the third form contained the preceding two (see Fig. 3). This device we hoped would lead *O* to relate the successive figures in the series and to note the 'compositeness' of the third figures in the sequences of three. Again the series was presented twice. Routine instructions were as follows.

A figure will appear for a very brief time. Observe and report exactly what you see. The report should include the course of your performance from the signal 'now' to the disappearance of the figure. Be sure to note any changes in your task as you proceed.

Since *O* was asked to draw certain of the patterns, the setting was altered from that in Series 1 by the following factors: (a) nature of the stimulus-objects presented; (b) the serial arrangement of the objects; (c) reproduction by drawing; and (d) a formal instruction to note changes in the task.[19] Although nothing is explicitly required beyond a description of the object, a hint is given that the serial order may be of importance in the request to note changes.

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new discovery. Külpe and others of the Würzburg School emphasized the *Aufgabe* and described its influence in the initiation of action and thinking thirty years ago.

[19] Cf. O. F. Weber and M. Bentley, Relation of 'instruction' to the psychosomatic functions, *Psychol. Monog.*, 35, 1926, (no. 163), 1-16.

The performances initiated under these altered conditions are very similar to those described in Series 1. Identification remains the primary end, and the individual differences related to individual preparation again appear. There are, however, three marked alterations in the observational activity: (1) an increase in the noting of details; (2) the origin of a new task from the cumulative effect of sequence in the series (occasional instruction); and (3) alterations in the performance resulting in a new kind of product.

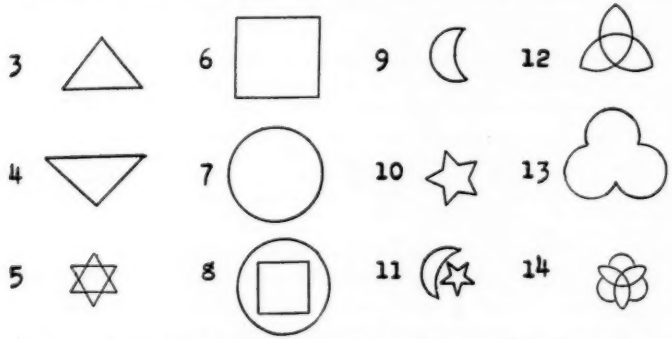

FIG. 3. THE SEQUENCES OF STIMULUS-OBJECTS USED IN PART II, SERIES 2

(1) Increase in noting details. The tendency to report more of the particulars of each presentation (*i.e.* to note more detail) usually appears. Two factors, the difficulty of finding names for geometrical designs (excepting triangles, squares, and circles)[20] and the occasional requests for drawings which put a premium on details, seem to direct this tendency. Many of the performances are typical inspectings.

Be: In the foreperiod I decided to watch all details, as well as the general form of the object. Saw a circle with smaller square inside. Could not determine whether the corners of the square cut the circumference or not. (8)
Co: Because I had to draw the last figure, I looked very carefully this time so that I might reproduce it. Saw a leaf-shaped figure with three principal parts. They were circular with points inward. Thought of the word, "fleur de lis." (13)

[20] The exact importance of labelling objects for their identification is difficult to determine from our protocols. Three *Os* (*Fn, Je,* and *Mac*) report a verbalization of a tag in the course of almost every performance, two (*Hu* and *Gl*) only occasionally, and the others almost never. There were instances in Series 1 in which *O*'s scrutiny appeared directed entirely toward abstracting aspects of the object which would aid him in supplying a label. The report by *Fx* is similar. Were it not for these, we should entirely concur with T. V. Moore (Image and meaning in memory and perception, *Psychol. Monog.,* 27, 1919, (no. 119), 85) when he writes, "One who looks squarely at the facts . . . will not be inclined to identify the imageless meaning with the process of naming an object. If this were the case, meaning without the word would be impossible and simply could not occur. But it does occur and that, too, rather frequently."

Gl: Again I was set to take in as many details of the figure as I possibly could so that I might draw it correctly. As soon as the object appeared, I said, "It's a triangle." Then I took note of the exact position. The base of the triangle was uppermost and the apex down. (3)
Je: This time I was going under the instruction, "I must look for all of this design." A complex something came on. I couldn't name it, but I looked carefully to fix the details. (15)
Fx: That was a figure consisting of several arcs of circles. I tried to name it; I couldn't, but I felt I had a very good picture of it. (12)
Mac: I was set to get every part of the design. First, I saw it as an extremely complex figure with a square standing out at the center. I analyzed the memory-image, and in so doing discovered not only the square but also a longer rhombus outside. (19)[21]

There are frequent instances, however, in which the details appear from *O*'s reports to have announced themselves, *i.e.* to have issued from simple perceiving.[22]

Be: A square with cross-lines. Lower line not visible. (2)
Hu: At first just the form of a geometrical design, then the darkness of the lower line stood out. Following that I noted the two protrusions. (Special Series 2)
Fn: Figure was apprehended as a square. Then noted without comment the distinctness of the three lines other than the base. (6)

From these reports it appears that particulars may issue from either perceiving or inspecting. The mode of function (as was also found in Series 1) is determined not only by the task, but by the difficulty of the task for the particular *O* at the particular time.

(2) Origin of new task. The appearance of the peculiar sequences of three in which the third is a composite of the two previous figures gradually effects a change in *O*'s task, and consequently in his performance. The instruction from the occasion acts cumulatively, finally becoming a part of *O*'s preparation in the foreperiod. Three *O*s (*Be, Mac,* and *Ro*) call the change in their task 'expectation.' Others characterize their new preparation as 'a set to see.' The effect of this occasional instruction varies from *O* to *O*. Two failed to be influenced, but all the others were led beyond the apprehending of form and details, beyond even scrutinizing the perceived object for

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[21] *O*'s occasional continuation of his scrutiny into the post-exposure period, where he examines the perceptual residues in an effort to satisfy his determination, indicates that the human organism is capable of inspecting objects apprehended memorially and imaginationally as well as those perceived. This fact invites investigation.

[22] Our interpretation here is at variance with that of C. Stumpf (Erscheinungen und psychische Functionen, *Abh. d. pruess. Akad. d. Wiss.*, 1906, Sec. IV, 20.) when he considers the noting *(Bemerken)* of the members of a tonal triad to be a 'mental function' distinct from perception. The difference in the impression given when the triad is taken as a unit and when it is analyzed could not be interpreted, he argued, according to the theory of qualitative transformations. Therefore the difference was accountable only by assuming a separate function of 'noting.' Stumpf's description of the analyzing of a tonal complex, the hearing out of components, undoubtedly involves at times just this scrutinizing which we call 'inspection.' But the noting of a detail in our setting may involve either inspection or merely a separate perceptive act. It should be mentioned here that Stumpf's functions were 'mental,' not modes in which an organism may act, and that he did not distinguish act from product. His argument in this instance appears to consist in defining a new function in terms of the product.

purposes of identification, to relating the object to those earlier in the series.[25] Here are reports.

Be: In the foreperiod, slight tension with the expectation that the figure would be in someway similar to those before. Then appeared the half-moon with the star in front of it. The star was smaller than the one that appeared before. (11)

Observer *Be*'s next report illustrates the cumulative effect.

Be: Tension, expected to see objects related to those before. Then appeared a figure taken as three ellipses overlapping in the middle. Sought some analogous object with which to compare it. (12)
Mac: This time the set included the expectation to see a somewhat similar figure in a slightly different position. I have been definitely influenced by the last three presentations. (15)
Ro: Since the last figures seemed to come in the order of two simple figures and then a complex one made up of the two simple ones, I am expecting to see that same order. (16)

The three following reports were given by *Je* after he had automatically found two composite figures.

Je: This time the design was apprehended as a triangle showing a slight blur at the lower right corner. Just as the exposure was closing there was a query, "Well, I wonder whether he's going to include this too in a later design?" (14) This was apprehended as a triangle having its apex at the bottom, and it was immediately followed by "Hmm, that was a triangle upside down." (15) This was very curious and complex. Before the exposure-period, I was absolutely set to see a triangle upside down infringing upon a triangle right side up. I felt that it would be both pleasant and relieving when it was shown. When the design appeared I saw it immediately as a six-pointed star. There was a slight disappointment. Then, just before the exposure ended, came a flash, "Why, a six-pointed star is an interlocked upside-down and right-side-up triangle." (16)

(3) New product. This change in the task alters the performances by leading *O* (a) to compare, and (b) to relate the objects perceived with those already seen.

(a) The comparing usually (not invariably) involves an additional step in the act. There is a difference between the procedure occurring before the task has become a part of the preparation and that which occurs afterward. In the former the appearance of the object carries a relationship to a preceding object which provides the occasion for the comparing. The reference usually comes out in a comment; then *O* proceeds.

Be: Nothing in the foreperiod. A triangle appeared. Comment, "Another triangle!" Compared it to the last as to size. Particularly noted that the hypotenuse and the base were shorter in this one. (4)
Co: This time the figure was complicated. Noticed it was similar to the last one except that it had a square as background. (14)

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[25] The differential effects upon *O*s of such occasional instructions as these may offer a method for distinguishing individual types. Most of the psychological differences between persons, as distinguished for the purposes of everyday living and as described by novelists, are the result of extra-organic and biographical factors (the beliefs, the residues of childhood experiences, etc.) which govern them. With careful planning, such a method would seem much more promising and practical in determining types of men than intuition, questionary, or test.

Gl: As soon as the object appeared, "Oh, another triangle." Then I noticed that it, too, was an isosceles, if not equilateral. (4)
Je: The initial thing this time was that my eyes were pulled down along the central axis with the result that I was looking at the lower apex of another equilateral triangle. Just before the exposure ended, there was a foreshortened verbal query, "Is that the old triangle upside down?" (4)

Occasionally, as in the following report of *Mac,* the reference and the comparison appear to be contained in one stage of apprehending. Such reports come after some of the composites have been automatically noted but before *O* had developed an explicit task of looking for them.

Mac: A triangle. I saw it immediately as smaller than the previous triangle. At the same time I saw it as being slightly to the right of the fixation point. So far as I can determine those things were intrinsic in the original perception. They did not come afterwards. (4)

(b) These backward references also issue from the composite figures. *O* does more than apprehend the form and label it; more than pick out the details. He relates it, as he does in comparing, to the products of previous observing. The result, the product of this relating, is 'compositeness.' Like the comparing, this relating commonly involves a separate step in the performance.

Fx: That was a star-like figure. As soon as I saw it, it was perfectly clear and I put it into a category, but it didn't have any name, except that it had something to do with stars. Then I suddenly realized that I could characterize it as two triangles interlocking into a star-like form. (5)
Co: This was a composite of the two preceding designs, the three portions of circles and the three lenses. (9)
Je: This was apprehended immediately as a design of considerable complexity. Almost at the close of the exposure-period I remember commenting that this was undoubtedly the last design overlaid on the previous one. (10)
Mac: I saw it originally as a six-pointed star with various intersecting lines. Almost immediately afterwards, I suddenly saw it as the previous two triangles fitted together. (5)
Ro: Here the figure was a six-pointed star made by superimposing two triangles. I presumed that they were the two triangles just seen. (5)

Series 3. Observation of two slightly differing objects. The plan here was to lead *O* by means of occasional instruction to compare for difference two perceived objects. It was expected from the studies of abstraction that, once *O* had been induced to look for difference, his performances would invariably be of the typical inspective variety.

In the arrangement of this series, the members of the first 5 pairs of pictured objects were almost identical (Fig. 4). The members of the second 5 pair differed grossly in some respect. The third 5 pair were different in some fairly obvious detail, while the last 5 were again almost identical. Excepting a sentence to prepare *O* for two objects on each presentation, the routine instructions were similar to those in the previous series. They read as follows.

Two figures will appear in this experiment for the same brief time as that in the previous series. Observe and report exactly what you see. Include a complete

description of the course of your performance from the signal 'now' to the disappearance of the figures. Try especially to report any changes in your task as you proceed.

There were four resulting variations of activity.

(1) Simple perception. So long as the two objects were accepted as identical, *O* was satisfied to apprehend them and to ascertain their identity. These are examples of simple perceiving like those already found in Stage 1 of the performances in Part I and like those in which identification is immediate, from Part II, Series 1 and 2.

(2) Emergence of task. There are instances in which, as we had expected, the gross differences announced themselves to *O*. Most *O*s were satisfied merely to see and to

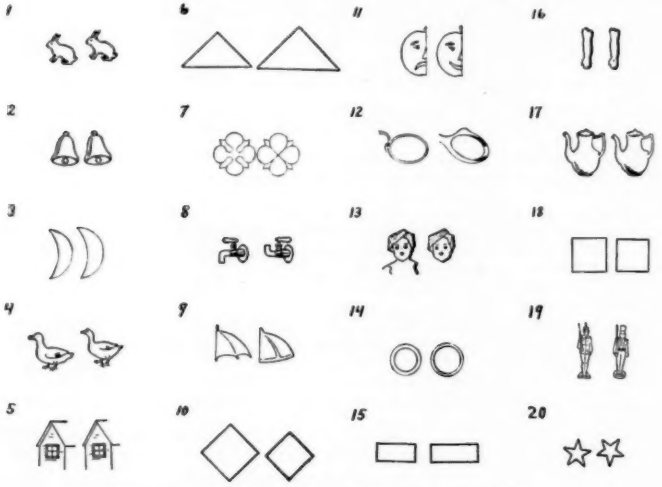

FIG. 4. STIMULUS-OBJECTS USED IN PART II, SERIES 3

identify the figures through the first 5 presentations, then one of the second 5 was apprehended as clearly unlike. Sometimes the difference appears to have emerged immediately.

Je: No reportable task. My first impression was that of a larger square standing on one corner and to the left a smaller square. This broke over sharply into a second stage which was highly verbalized, "The left is bigger; then there *is a difference* between these things." (10)
Mac: Two squares tilted on one corner appeared this time. The left square appeared decidedly larger than the right, and both were outlined in very black ink. The apprehension of the two as different seemed to me to be immediate. (10)

At other times the difference announced itself in a separate stage of apprehension, becoming then the occasion for scrutiny which was cut off by the brevity of the exposures.

Be: Saw two stars, then noted that they were in different positions. The one on the right was tilted slightly toward the left. (1)

Co: I saw two water faucets, but the exposure was too short. I think one of them was turned upside down. (8)
Fx: I was looking at the left figure when they appeared. Saw it was a water faucet and immediately said, "The right one is also a water faucet, but it is a little different." Then I turned to the one on the right, and it did not seem to be clearly a water faucet. Comment, "Why, that might even be such a thing as a fly." (8)
Ro: Here were two bells, and it seems to me that the left one had a line or two that the right did not have. Just before they disappeared it seemed there was a little more to the left than to the right, but I was unable to check this before they disappeared. (2)

(3) Acceptance of task. Following the automatic emergence of a difference once or twice, the task to look for 'difference' is accepted. The transition between implicit determination to identify and this task is much less gradual than that found in Series 2. This is the manner in which the task appeared: 'Why, those two are different: I wonder whether they *all* are?' Here are reports of performances just following the noting of unlikeness.

Be: Slight tension and expectation that the two figures would appear similar but differ in some detail. Therefore, self-instruction to look for similarity or difference. (4)
Co: I have now come to the point where I am looking for differences between the two figures. (6)
Gl: At the 'now' I was expecting that the two objects would be very similar, although there might be some differences between them, and I was set to look for these differences. (3)
Je: For the first time there was a reportable task in the fore-period. It was in a highly condensed form, which, if verbalized would be, 'I wonder if the two are really different? This time I must look, not to see what the objects are, but to see whether or not they are alike.' (9)

(4) Observation under a task. After *O* had taken the task of finding 'difference,' he wore a more alert appearance. His reports contain descriptions of searching scrutiny and of strains.

Co: I am still looking for differences. I saw two pots similar to coffee pots, but I saw no differences in spite of the fact that I looked very sharply for them. (4)
Gl: Task to look for differences. Recognized the objects as two ducks, although an instant later I thought they were geese. After I had recognized them I noticed the two were not exactly alike. My task was then to find out exactly what these differences were. Before I could do this, the objects disappeared. (4)
Je: About half way through came a sudden but highly verbalized query, "Are these things different? Look, see what you can find out about them." Looked left and right; apprehended the two as different. Very curious perception. All I can say is that the right one was somehow rounder, fuller, and freer from minor obstructions about the head than the left. (19)
Mac: I saw them first as two equivalent stars. The set to see difference came in as before. I became uncertain. It seems that the direction of the points on the right star was not the same. There were two distinct cross-references. First, perception of the two stars as the same, and secondly, a searching to find out whether there was any difference. (20)

These performances resemble very closely those of the second stage of Part I. There are the queries (self-government), the searching with its attendant strains, and the attempt to abstract some aspect of the perceived object which will solve the queries. Any aspect of the object, any hint of dissimilarity, is utilized. The reports show this activity to be much more complex.

In a few instances, after *O* had found several unlike pairs and had come

to believe that all pairs were different (they were), he reported differences for which there was no basis in the stimulus-objects. *O*'s task and his belief may contrive to make his performance unreliable. This belief, a product of his functioning, is a potent factor in his government.

Be: Tension and self-instruction to look for similarity or difference. Then two figures, the fronts of houses, appeared. One window in each house and smoke coming out of the chimneys. Seemed to be a difference between the two figures in this respect. Was not able during the brief exposure to determine this difference. (5) [There were no chimneys and no smoke.]
Ro: Two pitchers. Apparently the right one had some lines around the bottom which the left did not. [No basis in the stimulus-object.] Now that I am taking the objects for granted I find it possible during the short exposure to note the points of difference. (17)

Series 4. Observation of two objects under formal instruction to compare. In order to determine whether and in what ways the performances involved in observing two slightly differing objects would be altered by a formal instruction to compare, the stimulus-objects in Fig. 3 were presented again, but in a new order, with the following instructions.

Two similar figures will appear in this experiment for a very brief time. Observe and report whether the figures are identical or different. Include a complete account of the course of your performance from the signal 'now' to the disappearance of the figure.

Under these conditions most of the performances were of the same variety as those (Series 3) following the emergence of the task from occasional instruction. Any aspect which could be abstracted was employed. False differences were reported again. Certain changes, however, did result. With a formal instruction to report differences, that task acquired an 'official sanction.'

(1) Differences sought. More of the reports describe performances aimed at finding differences.

(2) Individual differences among Os reduced. Divergent personal interests were ruled out because the formal task required every effort. The only outstanding individual variations remaining can be referred to *O*'s peculiar way of taking the instruction, his own personalization of the instruction. When *O* assumed that every pair differed, his reports indicate a greater determination to find the point of unlikeness than they do when he assumed some like and some different. Under the former assumption 'no difference' is reported as something akin to failure. The length of the exposure was too short to allow much scrutiny.

Fn: Two ducks. Shifted fixation, but failed to notice any difference. (16)
Gl: Task to find differences stood out. Looked carefully, but figures were either so similar or so complicated, task was not carried to completion. (9)
Je: I had not found any point of difference, but just as the exposure was ending, I had the impression that one stem was curved, the other straight. Whole organism was left in doubt. (9)

Under the latter assumption *O*'s reports of 'same' or 'identical' carried the same positive aspect as those giving a point of dissimilarity.

Co: Two half-faces or pumpkins cut out like faces. Identical. (6)
Mac: I saw them as stars and almost immediately afterwards commented to the effect that they were identical. (2)

(3) Urge to identify decreased. By increasing the importance of finding a difference, the formal instruction decreased the urge to identify the objects. For some *Os* identifying appears to have become purely secondary, as is evidenced by the fact that their reports include no mention of what the objects were.

Co: My first concern is to see whether the figures are identical or different. After that, I find I usually know what the objects were, and I may recall having seen them before. (15)
Be: Two figures appeared. Noted that the lines to the center seemed slightly broader in one. (9)
Fn: Figures appeared and there was strained fixation, then the figures disappeared. After disappearance I noted a projection from the top of the left figure. [*Fn* frequently employed the memorial after-image for both identifying and finding differences.] (15)
Je: The designs were apprehended after a slight delay during which I was searching. I was definitely moving to cover the whole design. The outcome was in verbal terms "same." (8)

This was less common, however, than hurried identification followed by a query reinstating the task to compare and then scrutinize.

Gl: The figures appeared. I saw them as loop-figures with a bow or something attached to one side of them. There was a hasty attempt to find a name for them. The meaning 'eye-glasses' came. Then I recalled my task to look for differences, and I attempted to carry it out, but the figures disappeared too soon. (10)
Je: The initial apprehension was accompanied by a flashing verbal "Two faucets. Same." Then I looked to the right as the searching began, and just as the exposure ended, I saw the right faucet as being turned up. (13)
Mac: Two elongated objects running up and down on either side of the fixation-point. I saw them first as blurred, elongated objects. Distinctly after the original perception the word "arms" came in. They appeared very similar if not identical. (1)

Occasionally, when *O* found identifying interfered with finding differences, he attempted to thrust aside the determination to identify.

Gl: As soon as the objects appeared, I momentarily questioned, "What are they?" Then a partial answer, "They look like portions of umbrellas, but they might be something else." Then I hurriedly reinstated the task. There was a flash of reproach for bothering about what the objects were. (11)
Hu: Two objects with the flashing query, "What can they be?" Then before I answered it, there came just a fleeting reproach, "You're like some of your *Os*, you have to find out what they are before you compare them." The remark is amplified in reporting, of course. (Special Series 6)[24]

[24] With so many stages of activity in so brief a period of performance, the actual turns of the operation must be considerably more foreshortened than verbalization in the reports would indicate. The *Os* speak of 'fleeting verbalizations,' 'postures,' 'flashes,' etc., in an effort to describe the fleetingness and brevity of the separate stages, none the less effective for their brevity. They are undoubtedly the same phenomena as the *Bewusstseinslagen* found in the researches of the Würzburg School. When they are regarded functionally, it is plain that they are an exceedingly valuable accomplishment of the human organism. They reinstate the task in the course of execution, embody the queries about objects, comment with various meanings, and even supply a topic in 'flash' form. Though usually non-verbal they appear to do almost everything achieved by language itself.

Series 5. Investigation of the appearance of difference. In a disconcerting number of the reports from Series 2, 3, and 4 unlikeness and backward references appeared to issue from *O*'s observational activity simultaneous with, or even preceding, the identity of the objects perceived.[25] Now if unlikeness may issue from the first perceptive act, even before identity, it may be the product of perceiving as well as inspecting. The results of Series 1 demonstrated that identifying may require *O* to inspect. It is possible that there need be no direct relation between product and mode of function. In order to obtain evidence with which to answer this question and to determine the various stages of observational activity at which unlikeness and likeness may issue, we increased the exposure-time to 250 sigma and included in the instructions a special request to note when the differences issue. Ten new pairs of stimulus-objects, similar in make-up to those of Fig. 2, were presented. The instructions were:

> Two similar figures will appear again in this experiment. The time of the exposures will be comparatively long. Observe and report whether the figures are identical or different. Try especially to note whether or not the differences emerges simultaneously with the object. Include also a complete account of the course of your performance from the signal 'now' to the disappearance of the objects.

The temporal order of the activity, as our reports show, is not fixed. Three variations occur.

(1) Difference before identification. The dissimilarity issues from the first perceptive glimpse, the objects being different before they are identified. This is infrequent, but it occurs.

Be: The difference between the figures came before they were clearly noted and classified. One was lighter. Then I saw what they were and noted that one had a taller hat. (7)
Fn: When objects appeared, I said, "different." Then I set myself to search for the difference, but the objects disappeared before they were clearly perceived. (6)
Mac: I saw two different objects within the same frame. There was at the moment of exposition a slight movement down toward the left. The whole picture seemed slightly unbalanced. Almost immediately afterwards I found myself definitely commenting on the fact that they were different, but there can be no doubt at all that in the original perception that unbalance implied difference. (4)

(2) Difference simultaneous with identification. The unlikeness issues simultaneously with the objects. Every *O* reported several performances of this sort.

Be: Two figures appeared, one darker than the other. Noticed that they were two girls, similar, except that one was wearing something dark. (10)

[25] This question is related to that involved in Grünbaum's criticism of Ebbinghaus' treatment of similarity and difference when the latter classified these relations as perceptions. Grünbaum found the relationships to be independent of the contents, and argued that, therefore, they were of a different status. Cf. A. A. Grünbaum, Über die Abstracktion der Gleichheit, *Arch. f. d. ges. Psychol.*, 12, 1908, 449-450. Laying aside the question of contents, which does not concern our problem, our evidence demonstrates that these relationships may be the product of either the perceptive or the inspective mode of function.

Co: Two insects. The left had its wings up as though in flight. Noticed difference at the time I saw the objects. (6)
Fn: The objects appeared as stairs and as somehow different. (1)
Gl: As soon as the objects appeared, I saw them as two dissimilar sets of concentric circles. (8)
Je: This time the objects were apprehended immediately as being different. I did not at any time have a clear apprehension of the objects except as different. They were insects. (6)
Mac: This time there appeared two distinctly different objects. The difference was an essential part of the original perception. One the left was a rooster, on the right a hen. (7)

With performances of this and the former type the unlikeness is always prominent. When the difference is slight, *O* must do more than simply perceive.

(3) Difference after identification. The difference emerges after identification more frequently than at any other time. The performances which follow this sequence of events are exceedingly varied.

(a) The difference may emerge while *O* appears merely to be waiting, just as identity came in Series 1 following a delay.

Fn: As the objects appeared I stared at them. Then, suddenly, I realized they were different. It was as if I said to myself, "Of course they're different," although I actually said nothing of the sort. With this realization of difference, there was no change in the appearance of the objects. (2)
Gl: I saw them as two isosceles triangles. Definitely later the right one came out as somewhat larger. (7)

(b) It may issue just as *O* had adverted from one figure to the other in the pair.

Co: Two doors. The left door had a black spot in the middle. This difference I noted after I had shifted from the right to the left. (9)
Fn: When I found myself looking at the figure on the right, the figure on the left was not very prominent. Then suddenly there occurred what may be described as a widening of the area of apprehension to include the other triangle. There was no change of any sort referable to the triangle itself. The triangles were now apprehended together as they had not been before, and with that came the apprehension of difference in size. (7)
Je: Two rectangles without any impressions as to difference or identity. A second stage, markedly unpleasant, "confound it, I can't see any difference." Then a third stage in which my view swung to the right, and there was a non-verbal flash, 'right one larger.' (5)

In performances of types *(a)* and *(b)* (they are fairly frequent), search appears to have been relatively unimportant in the activity. There is little self-direction. These appear to be transitional forms of activity between outstanding modes we have chosen to call perceiving and inspecting.

(c) A third variety of the performances from which unlikeness issues, following identification, is the typical inspective kind. In the following report *Je* compares it with the more passive kind.

Je: Two ducks, the left clearer than the right. The observation itself was simple and was followed by a flash carried in kinaesthetic terms which can best be represented as shoulders being shrugged, and which if put into verbal terms, would mean, 'I don't know.' Then I began to search and cast actively about for some difference, but was unable to find any before the exposure ended. This term search which I have used repeatedly, refers to a very genuine difference between this performance and certain others. In some of the others, (*e.g.* the pictures of the two girls), I do not do *anything* as an *O*. I can best describe the process by saying that the objects are

presented to me. When, however, I cannot find the difference, I begin to take an active part. I begin to push, to thrust, and I am sure that it is accompanied by a rapid more or less random movement of the eyes which carries very often a general kinaesthetic suspense. (16)

Our questions may then be answered as follows. Unlikeness may appear at any stage of the performance. It may be the product of perceiving, of inspecting, or of any of the several transitional modes.

Series 6. Comparing linear extents. In planning the experiments we had expected any situation requiring comparison to initiate inspective observation. To reduce inspecting to its simplest form, and to eliminate the complicating factor of identification, 18 pairs of horizontal lines, drawn end-to-end, were exposed. One line of each pair (1-in. long) was introduced irregularly right and left in the pair. The other member of the pair represented steps of 1/16-in., ranging between the limits 13/16 and 21/16 in. The instructions were:

Two horizontal lines will briefly appear. Observe and report any difference in the line-lengths which you can find. Give a complete account of your performance from the signal 'now' to the disappearance of the lines.

In view of the results of Series 5, the only value of the present series is substantiation. The resulting performances varied with the individual *O* and with the magnitude of the difference in length. Some were as bare and simple as any we have found. At first, *O* prepared himself to observe carefully. After discovering the ease with which he was able to distinguish between the lines, he ceased preparing. The determination to distinguish, however, remained operative. Here are reports representative of the earlier members of the series.

Be: In the fore-period self-instruction to look carefully for the difference in length. Two lines appeared. I noticed immediately that the one on the left was slightly longer. (1)
Co: Strain preceded the exposure; wanted to look carefully. As soon as the lines appeared I saw that the left was much longer. (1)

Later the following reports are representative.

Gl: Left longer. As soon as the lines appeared I saw the left as very long. (6)
Je: Left longer. Difference apprehended immediately, followed by flash, "left longer." (6)
Mac: Two lines appeared, the left longer. This difference was present in the original perception and I commented on it at once. (2)
Ro: Left decidedly longer. I simply look, and the line either looks longer or it doesn't. (4)

These reports represent those performances in which difference was most easily found. The difference came out immediately as did identified objects in the simplest and briefest observations found in the first setting of our experiments.

When the disparity between the lines was less, the difference did not issue quite so readily. *O* attempted to get a confirmatory glimpse of the lines before reporting.

Be: Right longer. As the difference was very small, I compared them before deciding definitely.
Co: Left not much longer. Shifting from one to the other. First I make a general comparison then shift from one to the other in order to pick out the difference unless the discrepancy is such that I get it immediately by looking at both lines together. (12)
Fn: Longer line on the left. Appearance of the lines, then a rapid sweep of the view over the line on the right, then that line seemed to become stabilized and to assume its definite length. (4)
Gl: Right longer. The decision came almost as soon as the two lines were clearly seen. Then careful scrutiny of the lines to verify this decision. (9)
Je: Lines of equal length. Although there was rapid oscillation between them, there was nothing to change that report. Certainty very low. (12)

These performances are as complicated as they become in this setting. Only occasionally does *O* query and then enter upon active scrutiny.

Summary of Part II. We return to the questions asked at the beginning of this section. Out of the wide variety of observational activity found one may still define the two distinguished modes, perceiving and inspecting. The former is characterized by its bareness and by the uneventfulness of the manner in which its products issue. Its product is immediate; it is given in the act.[26] The latter is characterized by directed effort. When inspecting, *O* is perceiving and searching under his own direction for some definite end. His performance consists in deriving from the perceived objects aspects which will answer his queries. Between these outstanding modes, however, there is a series of transitional forms. Although these are typical of neither perceiving nor inspecting, they have aspects which may be common to either.

With regard to the antecedents of these two modes of observation, it is obvious from our results that the human organism adjusts its performance to fit the requirements of the task and of the situation. A tendency is evident for situations requiring comparison to initiate inspecting more frequently than situations in which only identification is required. On the other hand, every varation of activity may appear in either situation. In the immediate present, any of the forms of observational activity may be initiated by the occasion, by formal instruction, or by the observer himself; *i.e.* any activity may be initiated by any form of instruction. The circumstances which determine the manner in which the observer will per-

[26] By employing the word *immediate,* we do not mean to imply that perception is characterized by a brief temporal course. In our experiments, that was true. Either the task was immediately solved by perception or the observer began to inspect the objects. In the apprehending of a prolonged event through any modality, however, one may continue to perceive for an indefinite period.

form are the same as those which determine the difficulty of the task for the particular observer. Some are bodily, *e.g.* the efficiency of his visual apparatus, the set of the moment, state of fatigue; some are biographical, *e.g.* *O*'s familiarity with the particular design presented; and some are extra-organic, *e.g.* *O*'s belief that all, or only some, of the objects presented are different, and his attitude toward the task. Bodily and biographical factors are not always available from our method, the method of report. We do determine and describe many of the extra-organic factors.

With regard to product, there is no correlation between the logical category of the product and the mode of function from which it may issue. The product is largely determined by the task. *O* will exert himself just enough to succeed in the task. The task, however, must be his task. It is not synonymous with the phraseology of the formal instruction.

Part III. Observational Performance in Certain of the Psychophysical Methods

In the psychophysical methods, observational performance is presumably brought under strict control. A specific task is given. The observer is requested to report the presence or absence, the likeness or difference, of a certain object or attributive qualification, *e.g.* a tonal quality, the perception of dual position upon the skin, or the intensive difference of lights. Even the way-of-taking the object is frequently prescribed. Thus the observer is instructed to discriminate sounds on the basis of unitary fusion, roughness, consonance, pleasantness, or the prominence of individual members, or to distinguish weights by considering the quality of pressure in the fingers, strains from the wrist, and sudden onset of the weight, the visual picture of weight-masses, etc. Again, the general attitude of *O* toward the experimental occasion, the experimenter, the stimulus-series, and the likelihood of identity and difference have to be taken into account. Finally, in repeating the same operation over and over in the course of the experiment, *O* becomes variously instructed by the course and outcome of his own procedures. The sequential changes in the method of limits, *e.g.* invites *O* to anticipate the outcome of each observation. Considering these factors in this methodology, as well as the accuracy and precision of the discriminations involved, the psychophysical methods appeared an excellent field for the investigation of inspective observation.

In employing these methods, we wished to obtain further description of the government of observational performance and especially to discover the effects of repetition upon the inspective mode of observation. The three methods chosen were limits, constant stimuli, and average error.

The first (limits) was attractive for the occasional instruction afforded by the gradual change of amount of difference as the series proceeds. The method of constant stimuli was employed because it offers the best opportunity to study the effects of repetition without many complicating factors, and the method of average error because *O* must there rely chiefly upon himself for the directing of his operations.

(1) Method of Limits. *O* was presented with pairs of horizontal lines, exposed in the tachistoscope used in Part II. The standard length was 1 in. The variable lines were graduated—as in Part II—by 1/16 in. from 13/16 to 24/16 in. Again the duration of the exposure was set at 100 σ.

Following the customary procedure of the method, 20 ascending and 20 descending series, arranged in sets of 10, were given. In the first and last sets, the standard was on the right; in the other two sets, on the left. Twenty series filled an experimental period. Reports were recorded on the dictaphone for each performance in the first and last (descending and ascending) series of each set of 10. Eight *O*s observed.[27] The instructions were as follows:

We are now to proceed according to the method of limits. Two horizontal lines will briefly appear. The standard is now on the right (left). Compare the two line-lengths, and report whether the line on the left (right) variable, is longer than, or equal to, the line on the right (left), the standard. When asked you will also give a complete account of your performance from the signal 'now' to the giving of the report.

As in Part II, Series 6, the difference between the extents was immediately apprehended for a little more than half of the presentations employed in the method of limits. As before, these performances consist of a single stage, and the outcome is automatically verbalized in the reports as 'longer,' 'same,' or 'shorter' variable. For the remainder of the presentations, inspective or transitional forms of observation were reported. There were four definable circumstances initiating inspection.

(1) Small differences. When the discrepancy was too small to be immediately apprehended, *O* proceeded to observe more carefully and not infrequently to inspect.

(2) Serial graduation. The occasional instruction inherent in the serial graduation of the discrepancy. This cue is an unavoidable factor in the method. When *O* was led by the changing amount of discrepancy to expect a change of report, he set himself for careful scrutiny.

Be: Expectation of equality, therefore, self-instruction to observe carefully. I scrutinized the lines. At first they seemed equal, but, although I cannot tell exactly what the cue was, I finally reported the right longer. (S2-A5-6)[28]

[27] *Be, Fn, Gl, Je, Mac,* and *Ro* observed throughout. *Fx* and *Co* completed only 20 series.

[28] In labeling the reports, 'S' stands for 'set,' 'D' and 'A' for descending and ascending. The numerals stand for the number of the set or the series; the final numeral designates place in the series. Reports labeled 'R' are special reports of 'uncertainty.'

Co: They are becoming nearly equal, so I looked very sharply. Exposure seemed short, but decided the right one was longer. (S2-A5-6)
Fn: Shorter. Expected equality. Immediate apprehension of equality with uncertainty. Confirmatory examination made right line seem shorter. (S2-D5-4)
Je: Non-verbal expectation of equality. With appearance the lines seemed equal. Although assurance was low, I had no desire to change after I had looked back and forth at them again. (S3-A1-5)

(3) Anticipation. Attempts to avoid the influence of the anticipation set up by the series. Two *Os* (*Gl* and *Je*) occasionally induced the inspective mode during an exposure with self-instructed attempts to avoid any influence from anticipation.

Gl: Equal. Set for equality. As lines appeared, I saw them as equal. Then there was careful scrutiny to make certain that I had not been unduly carried away by my set. For a moment I was uncertain, but found no difference, so reported 'equal.'
Je: Right shorter. Set to see right shorter, but it was originally apprehended as shorter. Then came a flash, 'but is it shorter?' Looked again, and, as my view shifted to the right, I saw it was actually shorter. (S3-A1-3)

(4) The unexpected. Finally, a failure to find what was expected initiates an inspective scrutiny. When *O* is led by previous exposures to anticipate one report and by the product of his perceiving to make another report, he is suspended between the two determinations. If time allows, there is a flash of query 'which'; then a scrutiny.

Be: Slight difference expected. They appeared equal, but after careful comparing and some vascillating, I reported right longer. (S1-D5-4)
Gl: Set for equality. Left appeared as longer. There was a moment of hesitation and a flash 'which,' then rapid scrutiny. Finally decided the left was really longer. (S2-D5-5)
Je: Left longer. Difference was apprehended immediately and was accompanied by surprise, so I looked again. Had not been set for a difference so great nor in this direction. (S4-A1-3)

Bases for uncertainty. Another result of the anticipation which *O* derives from the serial presentation in the method of limits is uncertainty of report. Although there are other bases, conflict between a determination set up by the preparatory anticipation and another from observation is the most frequent occasion for doubtful reports. Because of our interest in obtaining a functional description of 'uncertainty,' we requested *O* to give an account of every performance ending with uncertainty. From these reports, five bases can be distinguished.

(1) Inadequate apprehension. For some reason, *O* failed at times to perceive the lines distinctly.

Be: Lines disappeared so quickly that I could not see them clearly enough to report. (S2-R1)
Co: I have just looked at a pair of lines three times because I couldn't see them well enough to decide. I am either too tired or too bored to get the whole thing. (S2-R5)
Fx: Doubtful. Exposure very short; lines not clearly seen. (S1-A5-2)
Mac: Doubtful. Failed to see the lines well enough to report. (S3-R2)

(2) Change of impression. During scrutiny *O*'s impression may change once or oftener, setting up conflicting determinations for the report. Since the brevity of the exposure will not allow continued inspecting, his performance ends with a question.

Be: Two lines appeared, but I am not sure whether they were equal or not. First I saw the left one as slightly shorter, then got an impression of equality. Seemed to be some difference, but there wasn't time to determine what it was. (S1-R1)

Fn: When the lines first appeared the variable was apprehended as longer, but immediately thereafter the lines appeared equal. Left doubtful. (S2-R1)
Gl: I expected equality, and for a moment I saw them as equal. Then I became uncertain and scrutinized the lines more carefully, finding the left a bit longer. I hastily scrutinized them again, and that time they seemed equal. Following that the lines disappeared and the uncertainty continued. (S2-R2)
Mac: The two lines jumped out as equal. This was followed by a period of doubt during which I saw the left one alternately as greater and as equal. When the exposure ended, I remained doubtful. (S1-D1-6)

Anticipative apprehension probably is one basis for these changes of impression, but this cannot be shown definitely from our protocols.

(3) Thwarted expectation. As already described, the report anticipated from the size of the last difference and from the place in the series appears wrong with apprehension. Although the stage is set for further scrutiny, the brevity of the exposure will not allow *O* to resolve his conflict.[29]

Be: Expected the line would still be longer. Right line immediately appeared shorter. Since this was quite unexpected, I felt uncertain. But there was no time to compare them further. (S2-R6)
Fn: Lines apprehended as equal, accompanied by surprise. Then there was apprehension of the left as longer accompanied by doubt. (S1-R1)
Gl: Equal, I guess, or shorter. Expectation that the variable would be longer. Upon appearance, I was surprised to find that the variable was not as long as I had expected. This uncertainty led me to scrutinize the lines very carefully, but after the exposure ended, I was still doubtful. (S2-D5-6)
Je: I was set very definitely for a leisurely report of 'right longer.' When the lines appeared they were disturbingly alike, with even a slight suggestion that the left might be longer, and accordingly, I gave the report "doubtful." (S1-R3)
Ro: Here I cannot say definitely that the left line was longer; it may have been, but there was a conflict between what I saw and the expectation I had. (S1-D1-5)

(4) Employment of extraneous criteria. Although the reports are usually based upon direct visual apprehension of difference in length, other criteria occasionally entered. Two *Os* (*Fx* and *Mac*) at times employed movement, probably the result of the spread of illumination in the darkened chamber, as a criterion of difference. *Fn* occasionally apprehended the end of the longer line standing out; sometimes he employed that as the basis for his report. If these extraneous criteria happened to conflict with apprehension of length, uncertainty resulted.

Fn: First impression was of equality. Then as I looked, the outside end of the standard stood out sharply. That meant the other line was shorter. Then I asked myself, "Is this an ascending or a descending series?" but before I could answer the question, the two lines had disappeared and I was uncertain. (S2-R1)
Fx: Doubtful. The movement of one line and the depth of the other conflicted, so I was left without an unequivocal basis for report. (S1-A1-9)

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[29] Another result of this anticipation set up by the serial arrangement of differences is the negative report, such as 'no difference.' Such reports appear to indicate that *O* has failed to get what he expected, failed to accomplish what he was prepared to do. Here is a report: *Co* Thought the left would still be longer, but could find no difference. (S2-R3) This finding verifies the conclusion by S. S. George (Attitude and psychophysical judgment, this JOURNAL, 28, 1917, 1-37) that such reports as 'no difference,' 'doubtful,' and the 'or-judgments' involve an extra-serial change in the observer's attitude toward the stimulus-objects. To be more specific, we should say that the extra-serial attitude is a determination set up by the serial arrangement of the method to report one result. The product of observation conflicts. These 'extra-serial' reports result.

Mac: In the movement, the two lines spread equally. When they became fixed I saw the left as longer. Then followed doubt which continued after the exposure. (S2-D1-3)

(5) Loss of the task. In a very few instances, uncertainty appears to result from a momentary loss of the task. Uncertainty resulting from this circumstance is different from that resulting from a conflict of determinations. It consists simply in an inability to report anything.

Co: Guess I forgot what I was doing. I just saw lines, and have no idea whether they were the same or equal. This experiment is awfully boring, and I find it hard to keep myself at it. (S2-R5)
Je: Doubtful. The two lines were apprehended simply as two lines without any reference to likeness or difference, leaving me with an attitude toward everything which I can best describe as a big question mark carried in kinesthetic terms. (S1-D5-8)
Gl: Longer. This time I was not aware of any task or any expectation. As the lines appeared I guess I saw the variable as different, but the task had disappeared, and I had no report. Then suddenly it occurred to me, "Oh, it's longer." (S3-A5-6)

Effects of repetition upon the performance. In the course of the 40 series of the method of limits employed in this experiment, each *O* gave approximately 200 reports. Although these are few for a psychophysical measurement, they are sufficient to show the effects of repetition upon the observational activity involved. These effects appear in *O*'s reported preparation and in the reported course of his performances.

(1) The effects of repetition upon reportable preparation are two. *(a) O* ceased to reinforce the formal instructions with self-instructions. At first he cautioned himself before each observation to look carefully; later he entered performances with no self-preparation whatever. An exception occurs at the beginning of each new setting. Then *O* cautions himself for two or three performances to report in terms of the variable. Here are early reports.

Be: Self-instruction to look carefully and to report in terms of the line on the right. (S1-D1-1)
Co: I looked very sharply this time and took special notice of the left line, so that I could say whether or not it was longer. (S1-D1-1)
Gl: Longer. The task set by the instructions was definitely present, and I thought, 'this is going to be more difficult for me at first with the standard on the left.' (S1-D1-1)

Later in the experiment the following are typical.

Be: No definite expectation and no self-instruction. When the lines appeared, I saw the left immediately as longer. That is all I can report. (S4-D5-7)
Co: I just look now. It is all automatic and I don't have to think what I am going to do. (S2-A5-2)
Gl: Equal. There is no task. The whole thing has become so bare there is little to tell. (S2-A5-1)

(b) Very early *O* came to expect the outcome of each observation. At first this anticipation became a part of *O*'s reportable preparation.

Co: Anticipated equality, and they looked just about equal. (S1-D1-5)
Gl: Expectation of shorter. As lines appeared I saw the variable as definitely shorter. (S1-A1-2)
Je: The fore-period was characterized by a tenacious verbal image, 'right will be longer.' When lines appeared, the right was apprehended as longer without further comment. (S2-D1-2)

Later it drops out of the reportable preparation, but remains as is evidenced by frequent surprises at the outcome. At this stage of the experiment, this anticipation linked the separate observations of each series together into a connected chain beginning with the first presentation and ending with the change of report.

Gl: Longer. No expectation, but I was very much surprised to see the variable longer. The whole thing has now become so fore-shortened that I can say little about it. (S4-A5-1)
Ro: Shorter. The one on the right was shorter. The total act has become quite habituated. I simply look and report: that's all. I don't even have any expectation any more. (S4-A5-1)
Je: This was again one of those stripped observations where nothing is reportable. I simply found myself reporting automatically. (S4-D5-3)
Mac: I was set for equality. The lines jumped out at me as equal and the report followed automatically. (S4-D5-4)

(2) The effect of repetition upon the course of the performance may be described as a foreshortening. Strain is reduced. Less self-direction is necessary. The whole act proceeds quite automatically. The exposure of small differences, which early in the experiment lead *O* to inspect carefully in order to determine the discrepancy, later require only the briefest and barest perception. Except when *O* is surprised by what he sees, perception suffices during the latter series of every setting.

Our data are too scanty here to enable us to determine the effect of repetition upon the objective accuracy of the observational performances.

(2) Method of Constant Stimuli. In the method of constant stimuli, 5 auditory stimuli were presented by means of a sound-pendulum. The apparatus was arranged for equal intensive steps between 20° and 60°.[30] The customary procedure of the method was employed, and 100 presentations of standard and variable were completed in each experimental period. Reports of performance were required for the first and the last 10 presentations of each hour. Two *O*s (*Ro* and *Mac*) observed, *Ro* for 3 periods and *Mac* for 5. The instructions were as follows:

Two sounds will be successively presented. Listen and compare the two for intensity. Determine whether the second is more or less loud than, or equal to, the first. After you have announced the outcome, give a complete account of your performance from the signal 'ready' to the giving of the psychophysical report.

In this setting perceiving almost always sufficed. In the early part of the experiment, each performance was a series of separate acts. The signal 'ready' brought *O* into a tense readiness. Hearing the first sound brought an instant of relief, which was succeeded by another period of anticipation of the second, during which the second sound was imaginally apprehended as louder, as weaker, or as same. Perceiving the second sound released the tension. The report was merely a verbalization of what was given in apprehension. Here are reports of early observations.

[30] Five equally spaced intensities were obtained by means of the formula (intensity$=\sin^2 \theta/2$) given in Titchener's *Experimental Psychology*, 1905, vol. ii, pt. i, 81. Computed by this method, the five settings on the sound pendulum were: 20°, 33.5°, 43°, 52°, 60°.

Mac: With 'ready' I was thrown into a state of tension. I found myself anticipating a number of variously intense sounds such as I heard in the preliminary trials. The first sound came in as a sharp click. During the interval between the two, I anticipated the coming of the second sound. I heard it variously as louder, as equal, and as weaker. When the second sound appeared it seemed to come after those I had heard in anticipation, and I heard it immediately as equal. The report was simply a verbalization of what I heard upon presentation of the second sound. (S1-1)
Ro: Louder. Here there was no particular expectation, just waiting. After the first sound came, I just sat and waited again until I heard the second. When it came it was louder than the first. (S1-3)

Little is contributed by *O* to the government of these performances. Except at the very begining, when *Mac* imagines the sounds to come, *O* merely sits prepared, and the first sound comes, followed by the second. The occasion completely governs the course of his activity. The effects of repetition in this setting are to reduce the anticipative tension, and to ligate the separate perceptive acts. Each element comes to carry a forward reference to the next. Here *O* is becoming habituated.

Mac: My set is becoming much more automatic. I scarcely noted the 'ready,' and I have now come to accept the first sound. More and more I am becoming directed toward the second. This time the second jumped in as louder and stronger than the first. (S1-19)
Ro: Softer. Here the first sound was about normal, and the second softer. I find that a certain intensity seems normal, and I anticipate the last one before it comes. (S1-19)

Finally this forward reference binds the whole chain into one continuous observational operation.

Mac: First sound was scarcely heeded; I was concentrated altogether on the second. It jumped in as smaller, lower, and weaker than the first, and the report followed automatically. May I repeat, that the comparing is a single act. It does not involve a recall of the first sound or an imaginal comparison of the first with the second. The basis for the report is included in the apprehension of the second sound. (S5-3)
Ro: Louder. Here the first sound carried a reference to the second as louder; it was, I do nothing but sit here. (S3-5)

In order to ascertain roughly the effect of repetition upon the objective accuracy of the observing in this setting, we have computed the percentage of correct reports given for each pendulum-setting in each experimental period. Although there are a few reversals, the results (Table II) indicate a definite tendency for the accuracy to increase from period to period. Since our data were so few, further statistical treatment was considered unwarranted.

Uncertainty and surprise. In this setting only two circumstances led to reports of uncertainty.

(1) Thwarted anticipation. As individual sounds came to lose their identity, they were apprehended as 'normal' or as louder and softer than 'normal.' Thus, anticipation also comes partially to determine the reports in the method of constant stimuli. At this stage of the experiment, *O* anticipates the outcome from the heard impression

of the first sound. Usually he anticipated correctly. When the second sound turns out to be just slightly different from what was anticipated, uncertainty results.

Mac: The first sound carried a reference forward and downward. The second seemed to jump in as a trifle higher, and I was immediately uncertain. If I were forced to report, I should say 'louder,' but I cannot be sure. (S3-10)
Ro: Doubtful. The first was louder than normal and caused expectation of a weaker sound. When the second sound appeared as louder than expected, it seemed equal, but I could not be certain. (S1-12)

(2) Extraneous criteria. Although *Ro*'s reports indicate that he employed only the heard intensity of the sounds, *Mac* frequently deserted the formal instruction and fell into a 'stimulus error.'[21] His familiarity with the apparatus led him to take the sounds as objects, *i.e.* as the pendulum striking the block more or less forcefully.

TABLE II
CORRECT REPORTS IN EACH PERIOD
(in percentages)

	Period	Pendulum settings*			
		60°	52°	35.5°	20°
Ro	1	65	25	30	90
	2	85	50	70	100
	3	90	45	60	100
Mac	1	70	40	70	100
	2	90	45	80	100
	3	100	60	75	100
	4	80	65	75	100
	5	100	70	80	100

* The standard was set at 43°. Each entry gives the percentage of the 20 comparisons of each variable with the standard correctly made for that period.

Mac: Throughout this morning's observations I have been troubled by the outstanding objectivity of the sound. I am not really reporting on sound-intensity. I hear the sound as a more or less forceful stroke of the pendulum. The terms 'loud' or 'faint' do not adequately describe the difference I am hearing. My experience is predominantly visual, although, naturally, the auditory factor is very important. (S3-R1)

He was also at times confused by pitch.

Mac: The second sound jumped into place slightly below the level of the first. It differed in intensity in that it seemed a trifle muffled. At the same time there was a difference in pitch which bothered me. After the report, I was uncertain. (S4-10)

Surprise is the outcome when the heard intensity of the second sound is so grossly different from what was anticipated that it stifles the deter-

[21] Maintaining the attitude requested in the formal instruction is frequently very difficult. As A. C. Reid (The effect of varied instruction on the perception of lifted weights, this JOURNAL, 35, 1924, 53-74) has demonstrated, some attitudes are more easily maintained than others, and even a highly practiced *O* cannot be trusted to maintain a difficult attitude continuously. O. D. Anderson (An experimental study of observational attitudes, *ibid.*, 42, 1930, 345-369) demonstrated that no *O* could maintain one way-of-taking an object for a period of 5 sec. An *O* can, however, report any shifts in his attitude, and quantitative work requiring a fixed attitude must make use of *O*'s reports in determining unacceptable reports.

mination for the report set up by the anticipation. The situation requires *O* to make a functional shift, thwarts his expectation. This turn of affairs appears to constitute most instances of surprise.

Mac: The standard carried a reference forward and upward to a louder sound. Consequently, when the second sound came, I was surprised to hear it as considerably weaker than the first. With the second sound there was a decided movement downward. (S2-9)
Ro: Softer. The first sound was weak. Expected the next might be louder. Was a little surprised to find that it wasn't, but there was nothing to do about it, so I just reported it. It was all quite routine except for the surprise. (S3-2)

(3) Method of Average Error. Since the performance involved in the method of average error is unlimited in time and must be directed in its course by *O* himself, this method afforded an opportunity to study another aspect of observation. *O* was instructed to equate the variable on a Galton Bar to a standard extent of 100 mm. Two *O*s (*Gl* and *Fn*) observed, *Gl* making 150 adjustments and *Fn* 100. The experiment followed the customary procedure of the method except for the reports of performance requested for about every sixth observation. The instructions follow.

We shall now proceed according to the method of average error. You are to set the variable equal to the standard by pulling the strings. Pull the variable back and forth until you are satisfied that the two distances are equal. When you have completed the adjustment of the variable, give a complete account of your performance.

The task of adjusting the variable in this method requires action as well as inspecting and perceiving. A difference in *O*'s approach to this as compared to those of the other psychophysical methods is immediately apparent. He approaches the first, and many subsequent, adjustments with a plan.

Gl: At the 'ready,' in addition to the task set by the instructions there was preparation on my part in subvocal speech. "Do this carefully. I must sit just right, and look directly at the mid-line. This is not going to be easy." (S1-1)
Fn: Considered how to make the adjustments. Saw the extents were obviously unequal, so I picked out the right string and pulled it to make the two extents look equal. (S1-1)

As *O* proceeds, his performance is governed both by his self-instructions and by the products of his scrutiny. More remote antecedents of function, *e.g.* habitual standards of excellency, are also factors which control his performance. They keep him going until he has made the adjustment as accurate as he possibly can. Some of these first adjustments required as long as two minutes. Consider the rest of the two reports quoted above.

Gl: When I began moving the variable along I felt that I was likely to jerk it too far, and I continually instructed myself to pull it very carefully. After pulling for some distance, I stopped and scrutinized the two distances. Deciding the variable was still too long, I pulled it a little further. This occurred again and again, during

which time I was commenting to myself subvocally. I questioned, "How do you know when you have it right?" Several times I thought I had achieved equality, then I would twist my head a little and look at it, and it wouldn't seem quite perfect. I finally decided, "Well, it looks all right, but probably it isn't." (S1-1)
Fn: I pulled the strings continuously until the extents looked equal. Then I scrutinized them carefully, particularly the variable. This scrutiny involved considerable eye-movement and strain. Equality was imperfect; the variable looked long. I pulled the strings again and they looked equal. Then I instructed myself to pull the strings until the variable was definitely longer and work back again. I did this, and then continued to diddle with the thing because the impression of equality was extremely unstable. The termination came with the self-instruction, "This is good enough." (S1-1)

As in other methods, the principal effect of repetition—and it is more apparent here—is the foreshortening of the performances. *O* devises plans which will shorten and facilitate his work. One of the first of these is to pick out the point of equality and to move the variable immediately to that point without scrutiny. Then he scrutinizes during the small readjustments necessary to give the impression of absolute equality. Here are reports of later performances. *O* had made about 20 adjustments.

Gl: At the 'ready' I saw that the variable was too far to the right. I moved it rather hurriedly to the point where I thought it should be. Then I moved it back and forth carefully estimating the two distances with my eyes, using one eye mostly because I learned that I do better with one. There were the comments, "It's getting easier now," and "I wonder whether I always err in a given direction." (S3-3)
Fn: I picked out a point on the side of the variable and pulled the strings until it was there. The two distances didn't appear quite equal. Two corrections were necessary. (S2-4)

These methods change from time to time. Although *Fn* habituated more rapidly than *Gl*, no point was reached at which self-direction ceased to be reported. Stabilization of the activity, if that is possible in this setting, was never achieved.

The foreshortening from repetition is most obvious in the decreasing amount of time required for adjusting the bar as the experiment progresses. The average time for each setting of the bar was computed and is recorded in Table III. Each set consisted of 25 adjustments. The average time required for an adjustment in the third set is about half that required in the first set.

Again in this method there is evidence that the performance tends toward greater accuracy and precision under repetition. In the results of computing the average error for each successive set (Table IV) only one gross exception appears (*Fn*, 3rd set). It is not explained unless it is a 'position' error.

It must be mentioned that in this setting, where *O* was allowed sufficient time to resolve any conflicting determinations with further scrutiny, uncertainty and surprise did not appear in the reports at all.

Returning to the questions asked at the beginning of Part III, the ob-

servational activity involved in these three methods is, contrary to our expectations, less likely to be inspective than perceptive. The mode varies with the nature of the task, with the difficulty of the task, with the circumstances of the particular technical methodology, and with the stage of practice.

Repetition in each of the methods reduced the amount of self-direction, foreshortened the operation, increased the precision and accuracy of the

TABLE III

AVERAGE TIME REQUIRED TO ADJUST TO EQUALITY IN SUCCESSIVE SETS

(time in sec.)

O	No. of set					
	1	2	3	4	5	6
Gl	23	16	12	15	14	11
Fn	33	16	13	12		

performance, and in a few instances actually effected a change of mode. Differences in line-length which at first initiated the inspective mode were later ascertained by perceiving. It is this telescoping which undoubtedly accounts for the decrease in the complexity of the 'structural pattern of consciousness' found in earlier studies of comparison, judgment, recognition, and abstraction.[32]

TABLE IV

AVE. ERROR BY SETS

(Error in mm.)

O	No. of set					
	1	2	3	4	5	6
Gl	4.00	3.10	1.64	1.10	1.56	1.56
Fn	5.40	3.36	7.20	2.72		

The psychophysical report has commonly been considered a 'judgment,' but the psychological status of judgment has usually been equivocal. Sometimes it has been an explanatory agent or concept.[33] In his attempt to obtain a psychological basis for judgment, Marbe defined it as a "conscious

[32] *E.g.* S. W. Fernberger, An analysis of the process of comparing, *Psychol. Monog.*, 26, 1919, (no. 117), 1-161; A. A. Grünbaum, Über die Abstraktion der Gleichheit, *Arch. f. d. ges. Psychol.*, 12, 1908, 340-478; and S. C. Fisher, The process of generalizing abstraction; and its product, the general concept, *Psychol. Monog.*, 21, 1916, (no. 90), 1-213.

[33] In his study of abstraction, T. V. Moore (The process of abstraction, *Univ. of Calif. Pub. in Psychol.*, 1, 1910, 175) writes that recognition involves a judgment or a suspended judgment because recognition is characterized by certainty or uncertainty. Thus judgment becomes an agent to which certainty or uncertainty may be applied.

process" to which the predicate 'right' or 'wrong' may be significantly applied, a logical not a psychological definition. He found no peculiar 'processes' marking it.[34] For Watt, working later in the same laboratory, judgment was a sequence of experiences transformed by a task or problem.[35] Seen from our functional standpoint, the judgment is merely a report, the verbalization of the products of observational activity, *i.e.* perceiving, inspecting, or any of the transitional modes.

Attempts to get a 'structural' account of these psychological phenomena led into numerous difficulties. Consider the uncertain or doubtful judgment. Since the prescribed list of 'mental processes' would not suffice for describing uncertainty, and other phenomena like and unlike it, a new class of elementary processes, the *Bewusstseinslagen,* was sanctioned. As these were considered to be the characteristic unanalyzable processes in the 'higher mental phenomena,' they challenged the ardent structuralists to resolve them into more elementary components. The attempt was really quite unsatisfactory as one can see by reading Clarke's study of the 'conscious attitudes.'[36] In her analysis of doubt and surprise, the attitudes most relevant to our study, she was forced to employ functional terms in description. In a functional setting their status is obvious; they are the products of function. Doubt or uncertainty issues when there is an unresolvable conflict between two determinations. Surprise results when a preparation to give one report is checked by what comes out of the observation. Such a description appears to us much more reasonable and more satisfactory than those in terms of an alleged 'existential process.'

Summary and Conclusions

An attempt has been made in this study to describe the human organism in terms of actual operation while it observes. Employing the method of report, the results have enabled us to describe (1) the modes of operation involved, (2) the factors immediately initiating performance, (3) the manner in which the functional operations are governed in their course, and (4) the products of function. No claim is made that these four aspects constitute a complete description of any psychological operation. To render the account complete, it would be necessary to consider the individual history of each observer, describing all those more remote factors in his life-course which have determined his biological and biographical

[34] Cf. Titchener, *Experimental Psychology of the Thought-Processes,* 1909, 81.
[35] *Ibid.,* 121.
[36] H. M. Clarke, Conscious attitudes, this Journal, 22, 1911, 214-249. It is obvious that many of the *Bewusstseinslagen* are flashes of comment, query, and self-instruction, by means of which *O* governs his operations in course.

career. It would also be necessary to describe all the somatic functions which underlie the psychological operations. Such a physiological description cannot, of course, be obtained from the method of report. Neither has it been supplied with precision or adequacy by physiologists and neurologists from present knowledge of the bodily organs and processes. It has not been a part of our task to adopt or to reject the various neurological hypotheses which are now current. It appears, however, that we shall scarcely obtain a working knowledge of cerebral and accessory functions until our psychological accounts have been improved and rendered much more precise than they have hitherto been.

The results of our experiments may be summarized as follows.

(1) There is a wide variety in observational activity. In that variety there are two outstanding modes, perceiving and inspecting. Between these are transitional forms of observation, less typical and outstanding, which are neither wholly perceptive nor wholly inspective.

(2) The perceptive mode is characterized by its bareness or the uneventfulness of the manner in which its products issue, and by the fact that it is governed in its course almost entirely by stimulus and by the occasion. A specific task may or may not be present.

(3) The inspective mode involves both searching and perceiving. It is characterized by self-direction and by some specific and well-defined task which cannot be automatically satisfied by perceiving. It consists in the attempt to derive from objects those particular aspects which will satisfy the current task.

(4) The antecedents of these two modes are not always different in character. Either may be initiated by any of the three forms of instruction. The circumstances which determine which mode is to be employed are the same as those which determine the difficulty of the task for the particular observer. Some are organic, *i.e.* the efficiency of his visual apparatus, the set of the moment, and the state of bodily fatigue; some are biographical, *e.g.* the observer's familiarity with a particular design, and some are extra-organic, *e.g.* the observer's belief concerning whether all or only part of the objects presented are different. It is the latter group of factors which are most readily determined by the method of report.

(5) The products of perceiving and inspecting cannot be differentiated. A tendency exists, however, for objects and events to issue from perceiving, and for relationships, symbolic significance, and general bearing to issue from inspecting. This distinction will not invariably hold, for even

identification frequently requires inspection, and such qualifications as 'difference' often emerge from perceptive observation.

(6) These observational modes contribute to understanding in two ways, first, by accumulating relationships and symbolic significance which the observer unites into a topic, and secondly, by contributing these same products to enrich the topic after it has been formed.

(7) The supposition that search is a generalized function, initiated whenever the organism sustains a task, has been verified.

(8) In the psychophysical methods employed (limits, constant stimuli, and average error) the variety of performance varies with the task, with the difficulties encountered, and with the stage of practice. The reports describe perceptive apprehension much more frequently than inspective scrutiny in all three methods.

(9) After repetition, observational performance becomes telescoped as action does. The amount of self-preparation and self-direction is reduced, and the performances proceed with more ease, more precision, and accuracy.

(10) The advantages of functional over structural description and interpretation are plainly exemplified in the accounts of uncertainty and surprise. Here conflicting determinations arise (a) from changing impressions, (b) from the opposition of plural criteria, and (c) from a disagreement between anticipatory preparation and the actual outcome of observation.

(11) We have limited our study to "the simpler and more elementary aspects" of observation. The broader and more complicated forms will well repay further investigation.

STUDIES ON CONTOUR: I. QUALITATIVE ANALYSES

By HEINZ WERNER, University of Michigan

The following investigations are intended to establish experimentally some facts concerning the nature of contour and its significance for the process of building up the optical object.[1]

I. THE SIGNIFICANCE OF CONTOUR

Experiment 1. Two figures are presented in stroboscopic succession.

FIG. 1. FORMS USED IN EXPERIMENT 1

The first figure is a solid black disk on a white background, the second, a black ring on a white background. The centers of the projected figures are seen at the same point. In the experiment made at Hamburg, disk *a* had a diameter of 30 mm., and the ring *b* an inner diameter of 30 mm. and an outer diameter of 40 mm. In the experiment using Dodge's tachistoscope disk *a* had a diameter of 15 mm., and the ring an inner diameter of 15 mm. and an outer diameter of 20 mm. It can be readily seen that both forms are so constructed that the contour of the first figure is identical with the inner contour of the second figure, in such a way that if both figures are given simultaneously they will be perfectly concentric.

The principal result of the experiment is the following: *Providing that there is a sufficiently rapid rate of succession, the black disk will disappear.* The factors which occasion this phenomenon are: (1) a repeated succession of figures continually observed (*i.e.* a/b a/b a/b, etc.); and (2) an optimum in the succession of the figures. This optimum lies within the range of the apparent movement of two figures, which are of the same size

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[1] These experiments were conducted partly by K. Wildhagen, who worked under my supervision, and partly by me alone. In all there were 15 *Os*. The qualitative results about to be discussed were gained by the use of the Wittmann stroboscope in the Hamburg Psychological Laboratory (cf. J. Wittmann, *Über das Sehen von Scheinbewegungen und Scheinkörpern*, 1921, 9). They were later subjected to a check using Dodge's tachistoscope (cf. R. Dodge, *Psychol. Bull.*, 4, 1907, 10-13), adapted for stroboscopic purposes at the University of Michigan, Psychological Laboratory. Two figures, one directly and the other as a reflection, are seen at the same place. Successive illumination of the two figures is achieved by the use of variable slits cut into a disk that rotates between the source of light and the experimental figure. I am greatly indebted to Professors J. F. Shepard and B. D. Thuma for this adaptation. The quantitative results, attained by the use of new apparatus in the Michigan laboratory, are to be considered in a second article.

as disk *a*, and whose centers are removed a distance of the radius from each other. In the almost instantaneous exposure of the Wittmann stroboscope the rate of succession of the two figures lay between 235σ and 130σ, and the pauses between each exposure of the pairs of figures varied between 1645σ and 910σ. The optimum conditions of the trials made with Dodge's tachistoscope are the following:

Exposure of *a*	Pause	Exposure of *b*	Pause
25-12σ	240-120σ	25-12σ	560-280σ

Within these limits the clarity of the phenomenon varied for one and the same individual (as does also the apparent movement). For different individuals the point of greatest clarity was specifically different. It may be noted that, by voluntarily evoking a certain attitude in observation, the black disk can be made to vanish within the ring even when there is a much slower rate of succession. Eight of the 15 *O*s were able, by the exercise of their volition, to see the ring without any darkened center, even at a pause of 300σ. On the other hand, when the rate of succession is too high, the ring becomes diffuse, although the phenomenon does not lose much in decisiveness.

The phenomenon, the disappearance of the disk upon reaching an optimum after a requisite number of repetitions, may be described thus: In 80% of all cases the ring, in the optimum, is seen to be black, and the field within the ring is seen to have the same degree of brightness as the field which surrounds it. In 7% of all cases the inner field is much whiter than the surrounding field (successive contrast?). In the remaining instances the inner field is seen to be somewhat gray. The optimum of the phenomenon —as the rate of succession rises from slow to rapid—is reached in the following stages: first, the black disk and the black ring are seen alternately; secondly, upon increasing the rate of succession, the inner field of the ring darkens; and finally, this darkness within the ring grows gradually brighter until, in the majority of cases, the inner field and the background show no marked difference, all trace of the black disk having now vanished.

Experiment 2. If black and white are reversed in the figures used in Experiment 1, the basic phenomenon does not change.[2] In the optimum only the white ring can be seen, the white disk having completely disappeared.

Experiment 3. If the succession of disk-ring is reversed to ring-disk, the disk will *never vanish,* and can always be clearly observed at *any* rate of succession.

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[2] The background in this case is, at the best, a very dark gray.

Experiment 4. If, in place of the circular figures, there are substituted angular, uniform figures (*e.g.* quadrates, Fig. 2), the results will be fundamentally the same.

This means that if the blank inner quadrilateral figure is presented first, and then succeeded by the quadrate that is the frame of the figure preceding it, the first figure will be found to vanish when a certain rate of succession has been reached. If the figures are reversed in the succession, so that the framelike quadrate is seen first and the inner figure afterwards, then the inner figure will never disappear.

The only difference between the experiment with angular figures and the one in which circles were used is that the optimum region is removed to a higher speed. This increase is evident in the reactions of all *O*s and varies between 10% and 30% more than the optimum rates cited for the ring experiment. A subsequent article will afford data that are quantitatively more specific.

FIG. 2. FORMS USED IN EXPERIMENT 4

FIG. 3. FORMS USED IN EXPERIMENT 5

Experiment 5. If irregular, angular figures, such as those in Fig. 3, are used, there are no basic changes in the results. Again, in relation to the ring experiment the optimum of succession is raised.

An explanation of these facts must begin with these following premises. (1) Every visual object, even when it is presented simultaneously in all its parts, is built up by a psychophysical process which, as a process, has a certain temporal duration. (2) Normally such a construction comes into existence because of a psychophysical difference in relation to the surrounding field, that is, the figure is conceived from the point of the strongest psychophysical difference, which is that of contour. Because of this, if some optical object is disturbed in such a way that, as a result of certain conditioning factors, the contour cannot be formed, then this optical stimulus becomes psychophysically ineffective. I mean this when I say that *contour has a fundamental significance for the optical perception of objects.* If this theoretical assumption is correct, the following must have occurred. In a certain succession of the two figures, of the disk and the ring, the process of forming the contour of the disk has been identified with the inception of the whole process of constructing the ring. The process of forming the contour of the disk will, therefore, be utilized in the building up of the ring (see Fig. 4). A specific, separate perception of the contour of the disk,

in consequence of this fact, is absent. Since this factor of greatest tension, the contour, is lacking, the whole picture of the black disk is also lacking. On the other hand, if the ring is presented first and the disk second, the ring, in this case, is already in the first stage of development, which permits the contour of the disk to be built up as a separate figuration. Therefore the whole disk can be seen.

FIG. 4. PROCESS OF FORMING CONTOUR

There remains still to explain why, in the case of quadratic figures, the phenomenon is occasioned with greater difficulty, *i.e.* why it occurs only at a much higher rate of succession. The ground for the difference in the behavior of the quadrangle and the circle lies, apparently, in the difference between the resistance offered by the solid square to absorption in the frame-figure and the resistance offered by the disk to absorption in the ring. Experiment 10 will show more precisely that angles are those parts of contour which have stored within them the greatest amount of psychophysical energy. At this point, the well-known experiments made by Hartmann may be cited.[3]

Hartmann demonstrated in tachistoscopic experiments that a circle exposed repeatedly at small intervals melted into a permanently seen figure much more easily than a quadrangle exposed in a similar mode of succession. He established further, that a quadrangle lying on one of its sides became one permanently seen figure more easily than if it lay tilted on one of its corners, in which case the angles would be more dominant. While the circle fuses into a permanent figure at a rate of succession of 125σ, the quadrangle expositions do not altogether coalesce until a rate of succession of 115σ has been reached.[4] We can, therefore, understand that, in our experiments, the optimum speed of succession which is necessary to absorb the inner solid quadrangle into the frame-figure must be higher than that necessary to absorb the disk into the ring.

A series of experiments support the theoretical explanation which is offered here.

Experiment 6. In the original experiment, if a disk is taken that is made up of black and white spots, instead of the homogeneously black, it will be found that it does not vanish even at a higher rate of succession. The inner quadrangle is also preserved when it gradually shades from black to white from the middle-point out to the edges, or, reversely, from dark to white from the outside inward to the middle-point. In all these cases there are formed levels of difference within the inner disk, or inner quadrangle (one might call them incidental contours), out of which is built the disk, or quadrangle.

Experiment 7. If the black disk is apparently smaller than the inner white field of the ring, in no case can it be made to disappear (Fig. 5).

[3] L. Hartmann, Neue Verschmelzungsphänomene. *Psychol. Forsch.*, 3, 1923, 319-396.

[4] These numbers signify the whole time of presentation: exposition 1 + pause + exposition 2.

In accordance with our theory this means that the solid black disk, in this case, has a contour that is independent of the ring, and therefore can be constructed as a completely self-dependent figure.[5]

II. The Dependence of Contour

The disk loses its visibility as soon as the contour, which might have built it up, is turned to use in the construction of the ring. On the other hand, should the ring lose its specifically ring-like character, the black inner disk appears again. This has been demonstrated by experiment. The ring

Fig. 5. Forms Used in Experiment 7

can lose its ring-like character in two ways. The breadth of the ring can become so small that the ring becomes a line-figure. Normally, one no longer sees a ring on a white field, but a white circle bounded by a black line (Fig. 6). Again, the ring can become so thick that there is no longer the impression of a ring, but of a disk, in the center of which is found a circular hole (Fig. 7). Accordingly, two kinds of experiments are possible.

Experiment 8. Wildhagen, in provisional investigations, has established the following facts. If the width of the ring (normally 5 mm.) is increasingly diminished, while the inner disk remains constant, a critical width of

Fig. 6 Fig. 7

Fig. 6-7. Showing Relation between Ring Character and Breadth

0.5 mm. is ultimately reached, at which point the solid inner disk begins to reappear. If it is diminished still further the ring (at a critical width of 0.25 mm.) becomes completely equivalent to a circular contour. This means that there is a rivalry between a black disk and a white disk. More precise quantitative results from this experiment will be given in the second article.

Experiment 9. A ring and an inner disk of the following sizes are presented: ring, $d_1 = 15$ mm., $d_2 = 2$ mm.; circle, $d = 2$ mm. (Fig. 7.) It then

[5] In reference to the "threshold of contour" as the maximum difference permitting absorption of the circle by the ring see the quantitative analysis in the second article, cf. also Experiment 27.

follows that the ring, providing the outer diameter remains the same while the inner diameter is relatively small and equal to the diameter of the solid inner disk, will not absorb the disk within the optimum succession of Experiment 1. At higher rates of succession (between $130\dot{\sigma}$ and 90σ) the following results appear: In 30% of the cases the black disk never disappears, in 50% of the cases it vanishes sometimes but now and then it will reappear. Only in 20% of all cases does it disappear completely upon increasing the optimum speed of presentation. In the light of our theory this experiment indicates that the contour of the relatively small disk in relation to the contour of the ring, quite contrary to the naïve expectation, is much more self-dependent than that of a relatively large disk. In order to understand this state of affairs it is only necessary to remember that the ring in Experiment 9 has more or less lost its specifically ring-like character, and has become a disk with a small circular hole in the middle. This means that the inner contour is now in a relation to the outer contour that differs

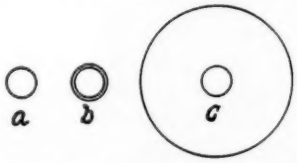

FIG. 8. SIZE-ILLUSION IN CIRCLES

from this same relation in the normal ring. The inner contour, together with the outer contour, no longer builds up a uniform, figural frame-construction,[6] but acquires a character more or less proper to itself alone (*e.g.* the character of being a hole).

If a small black disk (d=2 mm.) is succeeded by an equally small white disk making the hole in the black disk, then the conditioning factors do not differ greatly from those in a succession of a black disk and a white disk that is given alone. This means that the black disk appears in rivalry with the white disk. The contour is no longer used in the construction of the ring.

The fact that the diminution of the inner circle of a ring actually changes the whole figural relation becomes especially clear when we consider certain well-known phenomena in the realm of geometric-optical illusions (Fig. 8). If a circle of size *a* is drawn concentric to another slightly larger than itself, it will appear larger. If it is drawn concentric to a circle much larger than itself, the opposite illusion occurs, and it appears smaller.

[6] It may be said that the "parallelism" of the two lines of contour is now lost. The lines no longer proceed together.

Koffka,[7] discussing these same relations, justly remarks that this reversal of illusion goes hand in hand with the fact that in case *b* both circles build up a ring, while in case *c* the ring-character is lost. In other words, in *b* both contours are bound together into a complete unity of uniform direction, *i.e.* into a ring, and because of this there appears an assimilation of size. In the second case *c*, however, the outer contour and the inner contour are of a different quality. They stand opposed to one another as a hole-contour and a disk-contour. It must be assumed that there is a "critical" relation in the size of both circles, at which point the ring-character is metamorphosed into the other figural form.[8] Quantitative investigations of these phenomena are as yet lacking.

Whatever results our problem gives us will have to be checked by the quantitative investigations of the article which will follow this present one. It must be determined just what is the critical point in the size of the inner circle of the disk at which it begins to take part in the construction of the ring. It is exactly this relative size of the inner ring contour which will suffice to bring to the vanishing point a preceding disk of equal size in a stroboscopically arranged experiment.

III. The Energy of Contours

(1) Complete *vs.* Incomplete

Experiment 10. If the black disk of Fig. 1 is succeeded by a half-ring, instead of the full ring, the following may be observed. In the optimum, that part of the disk which fits into the half-ring will be absorbed. On the other hand, the semi-disk which has not fitted into the ring will appear increasingly dark from the inside out towards its unframed side (Fig. 9).

Fig. 9. The Phenomenal Appearance of Disk and Half-Ring Used in Experiment 10

Fig. 10. The Phenomenal Appearance of Quadrangular Forms Used in Experiment 10

From the standpoint of our theory this means that a part of the contour of the disk has been utilized in the construction of the ring, while the remaining part has developed its own contour, thereby permitting a portion of the disk to make its appearance. Analogous results are found in experiments made with quadrangular forms (Fig. 10). In these latter experiments it is also found that the inner quadrangle appears increasingly dark towards the unbounded sides. It will also be observed that the unbounded angle is distinctly dark. This, as we have already mentioned, points to the fact that angles are especially intense parts of contour, and that there is invested in them a central significance for the construction of optical figurations. Table I shows the maximal degrees of incompleteness in a frame-figure that will still suffice to absorb the disk completely in the construction of the ring. It represents these maxima

[7] Kurt Koffka, Psychologie d. optischen Wahrnehmung, *Hdb. d. normalen u. pathol. Physiologie,* XII, 2, 1930, 1255.

[8] *Ibid.,* 1256.

in terms of a frequency distribution. The results are gained from 40 separate trials made with 10 different *O*s.

It is to be inferred from these investigations that there is a certain limited degree of incompleteness in the frame-like figure which will leave the contour-energy altogether undisturbed. Here we can speak of a "threshold

TABLE I

SHOWING INCREMENTS OF COMPLETE DISAPPEARANCE OF DISK AT DIFFERENT GRADES OF INCOMPLETENESS OF RING

Am't incompleteness of ring	120°–101°	100°–81°	80°–61°	60°–41°	40°–20°
Increment	9%	16%	28%	35%	12%
Cumulative total	9%	25%	53%	88%	100%

of equivalence" in strength of contour relative to figures which show different grades of completeness. A certain ring, lacking a maximum portion of its contour, is equivalent in its figural strength to the whole ring. That maximum amount of portion which can be omitted without affecting the

FIG. 11. INCOMPLETE FRAME-LIKE SQUARE USED IN EXPERIMENT 10 (SIDE OF THE CENTRAL SQUARE 11.5 MM.)

contour-energy of the frame-like figure depends, of course, on the figural conditions.

If, therefore, quadrangular forms (Fig. 11) instead of circles are presented, the resultant conditions are essentially altered, as can be easily noted in Table II, a composite of the results of 30 experiments.

TABLE II

SHOWING INCREMENTS OF COMPLETE DISAPPEARANCE OF SQUARE AT DIFFERENT GRADES OF INCOMPLETENESS OF FRAME

Am't incompleteness of frame (in mm.)	3	2.5	2	1.5	1	0.5
Increment	15%	16%	26%	26%	12%	5%
Cumulative total	15%	31%	57%	83%	95%	100%

A comparison of Tables I and II shows, even before an exact reduction of the degrees to mm., that, on the average, a greater degree of completeness is necessary if the frame-like quadrangle is to absorb the middle square, than if the ring is to absorb the disk.

The following comparison will show this still more clearly. The maximum open section of the ring (measured from the inside) which permits absorption may be indicated by *x*, and the corresponding open section of the quadrangle by *z*. The total length of the contour of the quadrate figure, be it noted, is not so much less than that of the circular figure: disk diameter = 15 mm.; side of inner quadrate = 11.5 mm. In

every instance z reached only a certain percentage of x, as shown in the following comparison of results taken from experiments on 7 *Os*:

O	*A*	*B*	*C*	*D*	*E*	*F*	*G*
z/x	20%	25%	30%	32%	44%	52%	80%

This result is in accordance with what is already observed in Experiment 4. The inner square, because of its corners, offers greater resistance to absorption by the frame than does the disk. Angles are regions of the most concentrated energy. They give the figure a rigid contour and, therefore, a greater power of resistance.

A further phenomenon that is important for this theory of contour is the fact that the inner grayness of the middle field of the frame-figure becomes so much the darker the more incomplete the contour of the frame-figure.

Experiment 11. If two frame-like, quadrate figures are placed side by side, and are so constructed that the missing piece in b_1 measures 3.5 mm. in length, and that in b_2 7.0 mm., then the inner quadrate belonging to b_2 will appear darker than the inner quadrate related to b_1 (see Fig. 12).

FIG. 12. FRAME-LIKE SQUARES USED TOGETHER WITH BLACK CENTRAL SQUARES IN EXPERIMENT 11

This experiment, which will later undergo a more exact quantitative determination, permits us to make the following assumption. The weaker the contour (of the middle quadrate, resulting from a greater absorption by the frame-like figure), the weaker is the development of the whole field of figuration to which the contour belongs. With this we have reached the problem of "weak" and "strong" contours.

(2) WEAK *vs.* STRONG

Experiment 12. Strength can be added to the black disk of Experiment 1 by building it up within a more impressive figuration. By this means the strengthened contour of the central, solid disk is brought into competition with the absorbing power of the ring. Such a strengthened central figure is represented in Fig. 13. What occurs when this disk with radiating spokes precedes the ring? It is extremely important for the whole theory of contour that this radiating figure, under certain conditions, can be seen by all the *Os*. This means that the circular contour of this figure no longer is identified with the inner contour of the ring.

This does not mean, however, that, under certain conditions, the disk does not vanish normally for some *Os*. In general these following principal reactions (with the radiating spokes always visible) have been observed.

(1) The inner disk is brought to a point where it vanishes, but only upon reaching a greater speed than the normal optimum. In the normal optimum the circular part of the spoked figure appears gray, while the spokes themselves appear black.

(2) The inner disk may vanish within the optimum rate of succession. In this case the radiating spokes appear as parts of the frame-figure, that is, of the ring. Upon diminishing the speed of exposure, there can be seen a grayish center in a *ring figure with radiating spokes* (Fig. 14) that alternates with a ring that is white in the center and without radiations.

(3) The inner disk, even at a higher rate of succession is normally visible.

These three diverse modes of apprehension may be divided into the following percentage of frequency: 60; 20; and 20.

(4) In order to understand these conditions somewhat better, experiments have been made with those who normally react according to *(1)* and *(2)* in which *O* deliberately assumes a particular visual attitude. He is told to observe as closely as possible the figure with radiations as one whole figure. The succession is gradually increased from a slow to a rapid exposure. With these conditions, even at speeds greater than the optimum it is possible, at least temporarily, to induce *(3)* in all the *O*s, *i.e.* the inner disk remains visible.

FIG. 13. FORM USED IN EXPERIMENT 12

FIG. 14. RING WITH SPOKES

A typical phenomenon due to succession of Fig. 13 and ring

An explanation of these results must proceed in the following direction. The radiating form as the first projected figure can be effective either as an unbroken whole, or can be split up into a central disk and radiating spokes attached to it.

The experiments just mentioned, in which the *O*s purposely assumed a certain visual attitude in their mode of observation, are especially fruitful in clarifying the conception of the whole-figure, as such. It is quite illuminating that in these experiments the disk was particularly resistant to absorption by the ring. That, on the other hand, the middle figure can actually be separated from the radiations, is clearly shown by the observations recorded in (2). In this case the spokes are perceived *as part of the ring*, and not of the disk. Since in this case the relation between the radiating spokes and the disk has been lost, it is only natural that the disk is acted upon as if there were no spokes attached to it, that is, absorbed by the ring.

This explanation is supported by the following observations, recorded in Experiment 13.

Experiment 13. If the spoked figure (Fig. 13) is exposed along with a

half-ring in optimum succession, there will result, along with others more incidental, these two typical phenomena.

(1) One part of the radiations appear to be attached to the ring, while the other part of the spokes are still fixed to that portion of the disk which is not absorbed (see Fig. 15).

FIG. 15. TYPICAL PHENOMENON OBSERVED IN EXPERIMENT 13

There occurs, therefore, a separation of the radiations into two parts. Where the radiations are released from the disk, the disk has been absorbed. The spokes themselves appear to be a part of the ring. There, where the somewhat dark inner figure is still to be seen, spokes are also to be found attached to the disk.

(2) The same phenomenon is observable in this case with the following difference; namely, that part of the disk which is still to be seen is not dark, but a rather light gray. In this case the spokes are not only divided off into two parts, but are also *diverse in color*. The spokes on the ring are quite as dark as the ring, and the spokes on the remaining segment of the disk as light a gray as the disk itself. This shows clearly that the radiations are really so closely bound up with the two different figures that they even express the color of this unity. In all cases there is the same relation between the radiating spokes and the disk. In that instance where radiations and disk constitute a unity the contour is strong enough to withstand absorption by the ring, and in the other instance where the spokes are separated from the disk, the contour of the disk is too weak to be termed self-dependent.

The following summary can be made of all that has gone before. *The stronger the contour of the inner figure is made by its being built into a stronger pattern, the more this contour will resist absorption by its frame-figure.* Already one sees clearly what will receive further demonstration in subsequent experiments; namely, that *objectively identical contours which are parts of diverse total-figurations (as the central circle is a part of the radiating figure and of the inner contour of the ring) represent qualitatively diverse psychophysical processes which, as such, cannot be identified.*

Experiment 14. A further attempt to add strength to the contour of the inner figure consists in presenting this inner part as an "element" in a more complicated figuration. Here the models Figs. 16-17 are used. The frame-figure for the central figure *a* is a pair of rings *b*, which fit exactly over the small disks *1* and *2*. The frame-figure (Fig. 17*d*) for the central figure (Fig. 17*c*) is a single ring, concentric to the small circle *5*. Stroboscopic experiment with each of these pairs of figures give the following results:

(1) If *a* and *b*, Fig. 16, are stroboscopically presented, disks *3* and *4* and the pair of rings of *b* are seen alternately; disks *1* and *2* vanish in all cases. A similar

phenomenon occurs when only one ring, instead of the pair of rings, is presented. In this case one sees alternately, first, the small disks *1, 3, 4* and then, in place of disk *2*, the ring. In other words, the figure-constellation, in which the small disks are "elements" of the whole, is not strong enough to make the contours of the circles *1* and *2* independent of the rings.

FIG. 16 FIG. 17

FIGS. 16-17. PAIR OF FORMS USED IN EXPERIMENT 14

(2) Not quite the same results are obtained when *c* and *d*, Fig. 17, are used. In 75% of all cases the small disk *5* disappears when it is followed by the fitting ring *d*. On the other hand, in 25% of all cases there is an open competition between the building up of the disk in respect to the remaining disks of the constellation and the building up of the disk as part of the ring. This means that *O* often sees the central disk 5 even at a very high rate of succession, whereas it vanishes completely, at other times, within the optimum of succession.

If these experiments are considered all together it can be seen that the configuration *a* is ineffective, and in a majority of trials the configuration *c* is not strong enough to bring about the appearance of the small disk. If Experiment 13 is compared with Experiment 14, it becomes evident that, under otherwise equal conditions, the contour of a form which, as an essential unit, is uniformly built into a more extensive configuration, is more supported by this total figuration than is the contour of a form which is more or less a separable "element" in the whole of the figuration.

(3) PARTIAL LACK OR WEAKNESS

There is an essential difference between that case in which the figure has an incomplete contour, such as an open ring, and that case where the figure partially lacks any contour whatsoever. Fig. 18 may serve as an example of the character of a form partially lacking in any contour. If one

FIG. 18. EXAMPLE OF A FORM WITH INCOMPLETE CONTOUR

conceives the white space between the two black lines as a white band, one sees that it merges without the restraint of contour into the surrounding field.

Experiment 15. The difference between an incompletely closed, fragmentary figure and one that is partially lacking in contour becomes very

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clear in the following experiment. First of all, one has to recall Experiment 10 made on fragmentary contours. If a black quadrate is succeeded by the frame-like form of Fig. 10, the free angle of the central quadrate is seen to be very dark and to become gradually lighter in shade as it approaches the inner angle of the frame-figure. However, should the fragmentary quadrate now be represented by two parallels (Fig. 19) it will be found that, in most cases, the inner quadrate still appears as a flickering gray, although not with the same clarity as in Experiment 11.

Again, instead of the parallel lines of Fig. 19, which are parts of a quadrate, the pair of forms of Fig. 20 have been chosen. (Numbers indicate number of mm.) In this case, if *c* is made to precede *d*, then the first figure will vanish in practically all cases.[9] How can one account for the difference in effect between the experiment with the pair of forms in Fig. 19 and those in Fig. 20? This explanation might be offered. In Fig. 19 one is dealing with the contour of a quadrate which is strong in every direction,

FIG. 19 FIG. 20

FIGS. 19-20. FORMS USED IN EXPERIMENT 15

while in Fig. 20 there are "strips," which are weak, above and below, in contour. A strip or band, in contradistinction to a closed figure, such as that of a quadrate, does not have (psychologically speaking) two dimensions, but stretches out indefinitely in one direction without ever necessarily coming to an end. It will follow other figural laws than those applicable to the strongly closed form. The experiments have already shown this, in so far as the removal of only 3 mm. of one side of the frame-quadrate suffices, for many *O*s, to bring the central quadrate into view, while in this latter case a much greater section of the frame is removed without making the central figure visible. This can be attributed only to the fact that a quadrate has a strong contour on all sides, while the upper and lower contours of the central strip are either weak, or completely negligible. It is because of this fact that, in the pair of forms in Fig. 20, the edges of *c* do not appear although the frame-figure is open at this point. It could be assumed that it is the mere amount of the width of *c*, and not the whole figural character, which conditions the different results in Experiments 19 and 20. That the absolute width is not, however, the primary basis for this resistance,

[9] It makes no essential difference if the frame-figure is only 2.5 mm. in thickness.

that it is actually insignificant in relation to the strip-character and the closed figural character, will be made clear in the following control-experiments.

Experiment 16. The ideal form of an angular closed figure is obviously a quadrate. Now, the pair of forms in Fig. 19 is lengthened in such a way that the base is to the altitude as 2:3. Then it will be found that the results with these forms (Fig. 21) will be shifted from those attained in the experiment with the pair of forms in Fig. 19, in the direction of the results gained from the pair of forms in Fig. 20. The following

FIG. 21. FORMS USED IN EXPERIMENT 16

are the results taken from 12 *O*s. In 50% of all cases the inner figure vanished at a higher rate of succession (between 120σ and 80σ). In 25% of all cases there could be seen a washed-out, light gray figure with very poorly defined edges. Only in the remaining 25% of the cases were the *O*s unable to observe any difference between the present results and those recorded of the pair in Fig. 19.

There is no other possible explanation than that the increase in the height of the figure brought the form so much the nearer to its having the character of a strip, that is, the nearer to a form whose horizontal contours have been weakened. The vertical contours of the sides in Fig. 21 *a* are utilized in the building up of the frame-figure. Because of this the figure is robbed of its *essential contours,* and consequently disappears in most instances.

Experiment 17. If the altitude of Fig. 20 *c* is diminished, while Fig. 20 *d* remains constant, there is gained a maximum of clarity whenever the middle figure, Fig. 20 *c,* is quadratic (Fig. 22). In half of all the cases recorded, the small quadrate appears as almost black. In the remaining cases it is more or less gray, but it never vanishes.

FIG. 22. FORMS USED IN EXPERIMENT 17

FIG. 23. TWO CENTRAL FORMS USED IN EXPERIMENT 18

Experiment 18. Should one of the striated figures, illustrated in Fig. 23 (a_1 or a_2), be used in place of the middle figure Fig. 20 *c,* it will be found that never—no matter how great the rate of succession—are they absorbed by the frame-figure.

The ground for this phenomenon, as previously mentioned, (see Experiment 6), is that the central figure is constituted of self-dependent contour-parts, in consideration of which it can never be absorbed. It is notable, however, that while a_1 (Fig. 23) is always sharply visible, a_2 is much less clear, and does *partially* vanish in a distinctly predominant number of cases (80%). From this it must be inferred that the striations in a_1 have strengthened the horizontal contours, while those in a_2 have

weakened it, thereby permitting the strip-character, (relatively lacking of boundaries above and below) to appear dominantly. To a certain degree the results of the experiment with a_2 approach those of Experiment 15 with the pair of figures shown in Fig. 20 while by using a_1 the results tend in an opposite direction.

(4) Strengthening Partially Weak Contours

Just as it is possible to strengthen closed figures which are equally strong on all sides, in such a way that they become capable of resisting absorption by the contour of the frame, so, too, can this be done likewise with a "strip," or "band" character. This strengthening may be occasioned in two ways: (1) by strengthening those figural qualities which characterize the strip as a strip; (2) by building up such figural qualities as will make a relatively closed figure out of the strip.

Experiment 19. Strengthening a strip-figure in the sense mentioned in (1) is most easily accomplished by lengthening the altitude of a short band. Lengthening gives a stronger accent to the vertical direction, which con-

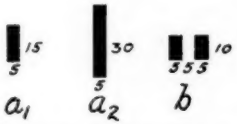

Fig. 24. Two Central Forms Used Alternately with a Frame-Like Figure (Experiment 19)

stitutes the strip-like character of the band, and consequently helps to build up the contours which bound the strip. This relation between the length of the strip and the strength of the figural and contour properties will be demonstrated in the following experiment. If *c* of Fig. 20 is gradually lengthened at the top until it becomes like the form illustrated in Fig. 24a_1 (Fig. 20*d* being unchanged), it will be found that a_1, when stroboscopically exposed, will vanish until it reaches a height of 12-13 mm., whereupon it gradually emerges in the gray, upper edge. If, on the other hand, a strip measuring 30 mm., is taken (Fig. 24a_2) whose length exceeds that of the frame-figures and projects equally below and above them (Fig. 24*b*), it will be seen by all the *O*s in its normal blackness.

Experiment 20. If, to the strip (Fig. 24a_1) measuring 15 mm., there are added obliquely posed striations (Fig. 25a_1), it is found that in 60% of all recorded observations this middle strip appears fully visible, while in the other 40% only that part of the figure to which are attached the striations remains visible.

It is apparent in this experiment that the contour is strengthened in such a way that a two-dimensional figuration is evolved from the one-dimensional strip. Naturally enough, the strongest contour is attained by taking a strip 30 mm. in length and attaching oblique striations both above and below. This figure (Fig. $25a_2$) proves to be absolutely resistant to absorption when thus strengthened.

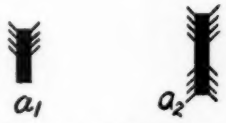

FIG. 25. TWO CENTRAL FORMS USED IN EXPERIMENT 20

IV. THE EQUIVOCALITY OF CONTOURS

Experiment 21. It is of greatest importance in an investigation of the problem of contour to determine the behavior of inner forms whose contours are equivocal, that is, which can be fitted into various patterns (Fig. 26).[10] It is easily recognizable that all these inner figures *a* are so constructed that their contours coincide with the inner contours of the frame-figures *b*.

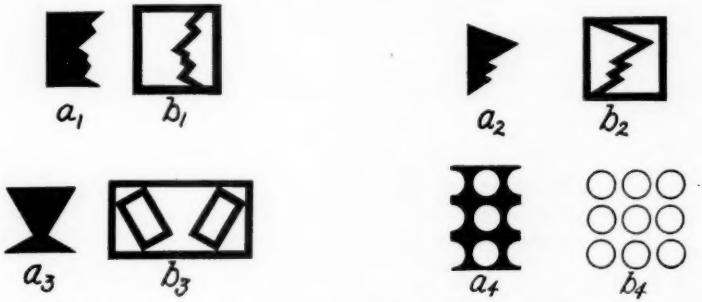

FIG. 26. FOUR PAIRS OF EQUIVOCAL FORMS USED IN EXPERIMENT 21

The following result is characteristic: The inner figure is always clearly visible at the optimum rate of succession noted in the basic experiment (Experiment 1). Disregarding, however, this constant fact, there can be observed these fundamental variations.

(1) The inner figure vanishes completely at a speed greater than the optimum.

[10] Cf. Erich Lindemann, Experimentelle Untersuchungen über das Entstehen und Vergehen von Gestalten, *Psychol. Forsch.*, 2. 1922, 41.

(2) The inner figure vanishes as such at a great speed of succession, but its place is taken by a more or less well-defined grayness.

(3) The inner figure occasions an increasingly strong impression even at the highest rate of succession (80σ).

The relations in percentage of these three modes of appearance are given in the following compilation of results taken from 30 trials with 10 *O*s.

TABLE III

SHOWING PERCENTAGES OF THE VARIOUS MODES OF APPEARANCE

Figures	Modes		
	(1)	(2)	(3)
a_1b_1	47%	20%	33%
a_2b_2	13%	40%	47%
a_3b_3	20%	53%	27%
a_4b_4	20%	47%	33%

The table shows that only the inner figure a_1 which, for a frame, has a sort of facial profile, is absorbed in about one-half the cases recorded. In all the rest of the experiments the inner figure is predominantly retained to some varying degree. It is also clearly indicated in the table that the contour of the inner figure a_2 is the one which, above all, offers the greatest resistance to absorption.

The grounds for the various behavior of the different contours can be made clearer by the intentional adoption of a certain visual attitude in observation similar to what has been used in earlier experiments. For such experiments the especially fitting pair of figures a_4 b_4 has been chosen. When, during projection, *O* at one time begins to see the disks and another time the jagged, interstitial figure in b_4, he is told to concentrate on this latter form. On such conditions the jagged black form will vanish for all *O*s, and for 50% of them the vanishing point lies within the normal optimum of succession. On the contrary, if the attention is concentrated on the disks in b_4, the black interstitial portions of a_4 will be seen in 65% of all cases, in 20% they will be seen faintly, and only in 15% not at all. These results are all the more striking since all uninitiated *O*s naturally expect that the black, interstitial figure will appear all the more clearly when the attention is focussed upon it, whereas just the opposite effect is produced.

All these facts are easily explicable from the standpoint of the theory of contour presented in this article. One cannot accept the "equivocal" contours as contours which are psychophysically identical and only "perceived" differently. On the contrary, these experiments demonstrate that the equivocality is based on a diversity in the real psychophysical process. When the contours at one time form disks and at another time a jagged form, then these apparently identical contours are diverse in an altogether real psychophysical sense. The simple evidence for this is that the usually predominant black, jagged figure a_4 behaves altogether differently, depending on whether it builds up the interstitial figure in b_4, or appears *beside* the ring contours

of b_4. In the first case a_4 is identified with the frame-contour, and in the second case it is not so identified, but exists independently next to the rings. The rims of the disks and the rims of the jagged figure are not psychophysically identical, but are qualitatively diverse. This is the most important evidence for the assumption that the equivocality of a figure is conditioned by the diversity of the psychophysical contour-processes. It is from this standpoint that one must grasp these experiments with a_4 and b_4. When *O* directs his visual attention on the interstitial part of the frame-figure, the conditions of the basic experiment, Experiment 1, are represented. That is, the contour of the black interstitial figure is used to build up the contour of the white interstitial figure constituting the frame. The results, too, approach those attained in the original experiment. The black interstitial figure will more or less vanish. If, however, *O* concentrates his attention on the circles in the frame-figure, conditioning factors are set up diametrically opposed to what has gone before. The frame-figure, b_4, has now altogether different contours from the mostly dominant, jagged contours of the first, inner figure, a_4. This first, black inner contour, which is qualitatively different from the frame-contour, is, therefore, able to preserve itself very well.

In order to understand the remaining results it is only necessary in every case to assume that the inner, first figure and the frame-figure become the more independent and separate, the more the figural conditions are provocative of a diversity of inner- and frame-portions in the successive figures.

The fact that figures a_1 and b_1, give results that differ from those gained from the three others is a divergence easily explained. Figures a_1 and b_1, because they resemble a human profile, gain in univocality, in decisiveness of contour. The "positive" black-contour, is at a disadvantage in respect to an apprehension of the inner figure, which appears parallel to the frame-figure, as a white profile on a dark ground.

On the other hand, in the pair of figures a_2 b_2 and also in the others, there is no great univocality of contour in respect to one another so that the "positive" contour of the black inner figure can more or less assert.

All these experiments demonstrate that objectively equal contours can be quite lacking in psychophysical equivalence. Now the problem may be posed in an opposite way. Is there psychophysical equivalence of objectively dissimilar, unlike forms?

V. The Equivalent Modes of Contour

The following stroboscopic arrangement was used in the investigation of the question as to whether objectively diverse contours can be psychophysically equivalent under these experimental conditions.

Experiment 22. Experiment 1 is changed in such a way that a regular polygon is substituted for the solid inner disk. These polygons have from 4 to 14 sides. The ring always follows the polygon in the stroboscopic succession. With an increase in the number of the polygon's sides and in the optimum succession of the two forms, the following successive stages were observed:

(1) Here there are but few angles. The contour of the polygon is clearly visible, the inner part is lighter, and the bounding angles very dark.

(2) The contour of the polygon is no longer seen as an independent form. The angles appear attached to the inner contour of the ring and the inner field varies from a medium to a light gray.

(3) Here there are many angles. The angles of the polygon now often appear also on the outer contour of the ring, so that one receives the impression of a ring with teeth. The inner field differs very little or not at all from the surrounding field.

These three stages with 10 *O*s succeed each other with lawful regularity as the number of the sides of the polygons increases. On the other hand, the number of polygonal angles characterizing each stage varies with the *O*s as shown in Table IV.

TABLE IV

SHOWING THE NUMBER OF OS OBTAINING EACH STAGE WITH THE DIFFERENT POLYGONAL ANGLES

Stage	Number of angles										
	4	5	6	7	8	9	10	11	12	13	14
(1)	10	10	8	2							
(2)			2	8	8	5	3	2	1		
(3)					2	5	7	8	9	10	10

These results contribute as follows to the theory of contour.

(1) It becomes evident that, with an increase in polygonal angles, the contour of a polygon becomes more and more "equivalent" to that of a disk and, consequently, the polygonal contour can be used more and more in the process of building up the ring, analogous to the manner in which a corresponding contour of a circular nature is used in this process.

(2) Angles are points of concentrated energy of contour, and, therefore, offer the greatest resistance to absorption.

(3) The fact that the inner field becomes lighter with an increase in polygonal angles is evidence that the more the contour loses in independency, the nearer its inner field approaches the vanishing point.

(4) The fact that in case of a marked equivalence of polygonal and circular contour *the angles of the polygon often appear attached to the outer rim of the ring* is excellent proof of the general theory that the contour of the central figure is actually used in constructing the ring.

In this experiment, indeed, much better than in the experiments with disks, it can be seen how the parts of the contour of the inner figure are drawn into the process of the construction of the ring. At the same time the coincidence between the appearance of the teeth at the outer ring contour and the more or less complete lightening in shade of the inner field serve to show the immediate, intimate relation between

the absorption of the contour of a form and the disappearance of the inner field proper to it.[11]

VI. The Structural Direction of Contour

It will be remembered that the effect noted in Experiment 1 was explained by the fact that the contour of the inner figure was used to build up the ring. When considering this phenomenon it is to be assumed, naturally, that the process of building up the ring works from the inside outward (see Fig. 4). This assumption is made on the basis of the responses of the *Os*, most of whom report a concentric movement of the ring from within outward. This assumption is supported by the results of the following experiment.

Experiment 23. If the inner disk is displaced in a sidewise direction, so that the disk is no longer concentric with the ring, these results will be obtained. Between the complete disappearance of the circle according to Experiment 1 and the stage of complete visibility (*a* and *b* being distinctly eccentric), there is a stage, when the disk is shifted outwards to the extent of 1 mm. at the right, where the disk will indeed be absorbed, but where the ring shows a bulge outward. The only explanation for this is that, in this case, there has been a non-concentric building up of the ring. This means that the process of ring-construction starts with the shifted contour of the disk; the disk has a larger influence on the displacement of the ring at that part where both figures are closer together than at the opposite part; therefore the distortion of the ring occurs at the right side (see Fig. 27).

Now the question arises as to whether it is possible for a ring to be built up by following some other direction than that afforded by the disk. What becomes of the disk in such a case?

Experiment 24. To answer these questions the material is arranged as shown in Fig. 28. A portion of the ring is exposed together with the disk. Otherwise the conditions remain the same as those in Experiment 1, *i.e.* the disk *a* (eccentric to ring fragment *a*) is concentric in respect to ring *b*.

[11] The technical apparatus used in these experiments is not sufficiently adaptable for use in the exact determination of color and contour. Because of this there can only be reported some preliminary results until more precise facts are made available by the use of special apparatus.

Experiments with inner and frame-figures of a *like* color give much the same results as those of Experiment 1. If an orange-colored disk is presented first along with an orange-colored ring, both forms being on a neutral background, the inner disk will vanish at an optimum succession. The completeness of absorption seems to depend on the colors chosen, on the relation between the color of the figure and of the background.

Experiments with inner and frame-figures of *diverse* color show interesting results. If a red inner disk is followed by a black ring, the red disk will not vanish even at the rapid rate of succession 80σ. This definitive result, which apparently depends on the relation of the colors chosen, has important consequences for the theory of the psychophysical contour-process, consequences which will be published later after all the color experiments have been completed.

When the two figures are stroboscopically presented, these, without exception, will be the results. With the same conditions in respect to succession which governed the basic experiment, it will be found that *the disk remains visible*. The ground for this visibility of the inner-figure can only be that the disk, in this case, is not needed in the process of building up the ring. It is not needed because the ring is built up much more from another side,

FIG. 27. DISTORTION OF THE RING IN EXPERIMENT 23

FIG. 28. PAIR OF FORMS USED IN EXPERIMENT 24

i.e. from the side of the ring-fragment. This explanation is reënforced by two observations. First, there is a jerking movement of the ring-fragment towards the ring. Secondly, the *O*s can cause the disk to vanish only by adopting a suitable attitude in their seeing (and indeed only after repeated attempts). They deliberately force themselves to see the structural direction "disk→ring," leaving to one side the fragment of the ring-figure.

The next experiments give corroborative results.

Experiment 25. A ring, whose inner diameter equals the outer diameter of ring *b*, is placed concentric to the black disk *a* (Fig. 29). When these figures are presented in succession, the following results are to be noted. (1) The black disk always remains visible (even at a speed greater than the optimum). (2) Moreover there can be seen a movement of ring *a* as it shrinks towards the size of ring *b*. This shrinkage is either complete, whereupon one can see ring *a* disappear into ring *b*, or it is incomplete, and the resultant ring will be of a size between rings *a* and *b*. The frequency

FIG. 29. PAIR OF FORMS USED IN EXPERIMENT 25

FIG. 30. CENTRAL FORM USED IN EXPERIMENT 26

of the occurrence of both is about the same. The relations in this case admit of the obvious optical interpretation, that the structure of ring *b* proceeds concentrically from the outside inwards in consequence of which the contour of disk *a* builds up its own circle and allows it to become visible.

Experiment 26. One can cause all the inner figures which have been used to reappear by changing the structural direction in an analogous manner. For example, if one sets a fragment of the ring about an inner form of Fig. 16, the small black disk at right, which would otherwise be absorbed by a ring, remains clearly visible (Fig. 30).

VII. The Unification of Diversely Located Contour Processes

Experiment 27. As previously shown in Experiment 7, the inner figure becomes visible when it differs markedly in size from the frame. A provisional analysis conducted by K. Wildhagen in the Hamburg Laboratory shows, however, that the disk (normal diameter=30 mm.), when diminished 2 mm., will still vanish. This means that the contour of a considerably smaller disk under the conditions of this experiment, is equivalent to the inner contour of the ring, and can be absorbed in the structural process of building up the ring. This problem of equivalence in size will be considered later in the quantitative experiments. What is interesting here is the mere fact that psychophysical unification of the contour-processes is possible, even when the stimuli collocation corresponding to both contours are different to a marked degree. This phenomenon is clearly expressed in the following experiments.

Experiment 28. This experiment proceeds on the basis of the first experiment. A black disk is followed by a black ring. The rate of succession is

Fig. 31. Forms Used in Experiment 28

decreased so far *beneath the optimum* that the black disk *is clearly seen.* The experimental conditions are now altered in such a way that with the ring there is also projected a black disk, *y*, almost tangent to, above, and to the left of the ring, which is of the same size as disk *a* (Fig. 31). If the visual attention is now fixed upon disk *y* during this relatively slow stroboscopic succession, it will be found that *disk a vanishes completely.* If the attention is focussed on disk *a*, it will reappear. The phenomenon becomes still more pronounced if two small disks, y_1 and y_2, are similarly placed near the ring, and projected simultaneously with it. By centering the attention on y_1, the limits of speed of succession at which disk *a* reappears may be determined. The duration of the periods during which the inner disk remains invisible are astonishingly long, as the following results (10 cases, 30 trials each) show:

	Intervals between successive exposures				
	280-300σ	301-320σ	321-340σ	341-360σ	361-380σ
% disappearance	1-10	21-30	31-40	11-20	11-20

There may be doubt cast on an assumption that the ground for such a persistent disappearance of the inner figure is really conditioned by the disk *y*. It might be assumed, for example, that the mere peripheral perception of the ring and disk *a* are at the root of the phenomenon. It is easy to demonstrate, however, that peripheral perception has facilitated to some degree, but by no means altogether accounts for, the disappearance of the inner disk in this particular case. If disks *y* are taken away, and one focusses a point above the ring, it will be found that disk *a* does not vanish at that speed of succession at which it vanished when disk *y* was present.

The results of Experiment 28 suggest that there is a unification of the contour processes of *a* and *y*. The contour process characterizing *a*, fundamentally weakened by the presence of the ring,[12] is absorbed by the strong contour process contiguous to it, namely, the contour-process *y*. Since we know from Experiment 27 that spatial disparity really does not stand in the way of identification, there is nothing to be said against the assumption of unification in this present somewhat crass instance of spatial diversity.

There might be another explanation, the possibility that the black disk *a* springs into disk *y*, and so vanishes from the ring. This explanation is supported by the fact that many *O*s report that they actually observe a jerking moving of *a* in the direction of *y*. In this connection it is well to consider, however, the following points:

(1) That it is quite pertinent to ask the question whether any phenomenon of movement is not fundamentally based on a unification of contour-process.[13]

(2) That, under the conditions of the experiments in which Dodge's tachistoscope was used, there is often observed a movement contrary to the expected $a \rightarrow y$, direction, that is: $y \rightarrow a$. The circle *y* jumps against the rim of the ring. This observation indicates that *a* and *y* do, indeed, belong together. In such a case it would be thinkable that the common contour was not built up psychophysically either in *a* or *y*, but somewhere on the way between *a* and *y*.

(3) That, in case y_1 and y_2 are given in place of *y*, the jerking movement is never observed, according to the observations reported by all *O*s excepting one. This negates any assumption that a movement of *a* in the direction of *y* is the basis of the disappearance of *a* in the ring.

That the phenomenon of the disappearance of contour *a* depends upon centering the attention of *y* is explicable in the light of an assumption that only a strong contour-process, strong because of the attention focussed on it, is able to attract the contour-energy of *a* to itself and to absorb it in the process of construction. The fact, furthermore, that it is only when *a* is relatively weakened by the ring that the phenomenon just discussed occurs is again demonstrable in the following experiment using colored figures.

[12] This relative debilitation is shown, for example, by the fact that the disk if focused, does not always appear dark, but is often gray, and at times, light-gray, in appearance.

[13] Cf. W. Köhler, Zur Theorie der stroboskopischen Bewegung, *Psychol. Forsch.* 3, 1923, 397-406. L. Hartmann, Neue Verschmelzungsphänomene, *idem*, 319-396. W. Scholz, Experimentelle Untersuchungen über die phänomenale Grösse von Raumstrecken, *ibid.* 5, 1924, 220-272.

Experiment 29. If red disks are substituted for black disks *a* and *y*, then *a* does not disappear (Fig. 32). In spite of the fact that *a* and *y* have the same contour, despite the undoubted tendency to the unification of *a* and *y*, the red disk *a* remains visible within the black ring. The tendency to unification just mentioned is shown in the reactions of some *O*s, who observe a jerking movement of *a* towards *y*, or of *y* towards *a*.[14]

FIG. 32. FORMS USED IN EXPERIMENT 29

FIG. 33. FORMS USED IN EXPERIMENT 30

Experiment 30. If *a* is of the same color as the ring, while *y* is red, then *a* will not disappear at this slow rate of succession. In this case the one condition of our experiment is fulfilled, *i.e.* the weakening of the contour of *a* by the ring, while the other remains unaffected. The contours of the red and black disks do not become identified. Because of this the black disk (below the optimum, naturally) still remains visible. We arrive at similar results when we use strip-like forms of Fig. 24.

Experiment 31. It will be remembered that in Fig. 24 only the uppermost part of strip a_1 (2-3 mm. over the frame-figure) came into view. Just

FIG. 34. PAIRS OF FORMS USED IN EXPERIMENT 31

as in the experiment previously discussed, one can attempt to take away the energy of the first figure by introducing contour-energy acting at a distance.

The experimental arrangement is as follows: Two constellations of forms (Fig. 34) each contain a black band 15 mm. long and 5 mm. wide, two smaller bands, 5 x 10 mm., spaced 5 mm. apart, and, above these, two lines, each 5 mm. in length. In constellation a_1b_1 these are separated by a distance of 15 mm., and in constellation a_2b_2 by a distance of 5 mm.

[14] As is already known from the results of experiments on color and contour, mentioned in footnote 11, there is no absorption of *a* into the ring and no diminishing of contour-energy when there is a difference in color between disk *a* and the ring. This present experiment demonstrates that *a* vanishes at a slow rate of succession, only when *disk y and the ring together absorb the energy of a.*

Constellation $a_1 b_1$, when the rate of succession is within the normal optimum, yields the following results. In 85% of all cases the strip a_1 vanishes completely should the spot x of b_1 be focussed by the O. In 15% of all cases it vanishes under all conditions.

When constellation $a_2 b_2$ is used, the strip vanishes in all cases with any method of focussing, providing the succession is within the optimum, and does not reappear for 40% of all cases even at a rate of succession of 300σ.

An explanation of these results is supplementary to the one given above. The line-contours absorb the weakened edge-contours of the strip. There occurs a unification of the contour-processes upon the introduction of new contours at a certain spatial distance. If the spatial distance is considerable, as in constellation $a_1 b_1$, the ability of the line-contours to absorb the edge-contour is small. Because of this there is absorption, in many instances, only when the factor of peripheral perception is introduced in order to weaken still further the strip-contour. If the distance between the line-contours and the horizontal edge of the strip is small (as in constellation $a_2 b_2$), then the strength of the line-contours is sufficient to absorb the strip's edge.

THE CONDITIONS OF REMINISCENCE

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Reminiscence is defined in this paper as the improvement in the recall of incompletely learned material after an interval of time without intervenient formal relearning or review.

This phenomenon was noted as early as 1903 by Henderson[1] and by Lobsien,[2] and in 1904 by Binet.[3] Binet stated that, although the phenomenon was not general, it had been observed so frequently that it was difficult to ascribe it to an error.[4] Subsequently reminiscence has been incidentally noted by Boldt,[5] Colvin and Myers,[6] Pyle and Snyder,[7] Norsworthy,[8] Myers,[9] Gates,[10] Guillet,[11] King and Homan,[12] Winch,[13] Gordon,[14] and McGeoch.[15] The number of references to such incidental noting of reminiscence could, no doubt, be easily increased, but such additions would help little, if any, in determining the factors of which reminiscence

* Accepted for publication August 10, 1933.

[1] E. N. Henderson, A study of memory for connected trains of thought, *Psychol. Monog.*, 5, 1903, (no. 23), 1-94, esp. 45, 88-94.

[2] M. Lobsien, Aussage und Wirklichkeit bei Schulkindern, *Beitr. z. Psychol. d. Aussage*, 1, 1904, 26-89.

[3] A. Binet, Sommaire des travaux en cours à la société de psychologie de l'enfant, *Année psychol.*, 10, 1904, 116-130, esp. 127.

[4] The following is the exact quotation "Cette sorte d'améloriation de la mémoire pars le temps, sans être générale, a ét é observée si fréquemment qu'il a paru difficile de la mettre en doute, et de l'attribuer à quelque cause d'erreur."

[5] C. Boldt, Studium über Merkdefekte, *Monatsschr. f. Psychiat. u. Neur.*, 17, 1905, cited by Adolph Pohlmann in *Experimentelle Beiträge zur Lehre vom Gedächtnis*, 1906, 84.

[6] S. S. Colvin and E. J. Myers, The development of imagination in school children and the relation between ideational types and the retentivity of material appealing to various sense departments, *Psychol. Monog.*, 11, 1909, (no. 44), 85-126, esp. 113 f.

[7] W. H. Pyle and J. C. Snyder, The most economical unit for committing to memory, *J. Educ. Psychol.*, 2, 1911, 133-142, esp. 140.

[8] Naomi Norsworthy, Acquisition as related to retention, *J. Educ. Psychol.*, 3, 1912, 214-218, esp. 216.

[9] G. C. Myers, A study in incidental memory, *Arch. Psychol.*, 4, 1913, (no. 26), 1-108, esp. 55; Recall in relation to retention, *J. Educ. Psychol.*, 5, 1914, 119-130, esp. 126 f.; Confusion in recall, *J. Educ. Psychol.*, 8, 1917, 166-175, esp. 171-174.

[10] A. I. Gates, Recitation as a factor in memorizing, *Arch. Psychol.*, 6, 1917, (no. 40), 1-104, esp. 46.

[11] Cephas Guillet, A study of the memory of young women, *J. Educ. Psychol.*, 8, 1917, 65-84, esp. 74 f.

[12] Irving King and T. B. Homan, Logical memory and school grades, *J. Educ. Psychol.*, 9, 1918, 262-269, esp. 268.

[13] W. H. Winch, Children's perceptions, *Educ. Psychol. Monog.*, 1914, (no. 12), 1-245, (esp. 50-238); Should poems be learnt by school children as 'wholes' or in 'parts,' *Brit. J. Psychol.*, 15, 1924, 64-79, esp. 76.

[14] Kate Gordon, Class results with spaced and unspaced memorizing, *J. Exper. Psychol.*, 8, 1925, 337-343, esp. 342 f.

[15] G. O. McGeoch, The retention of observed material, 1-47, M.A. thesis, 1926, on file in the library of the University of Chicago.

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is a function. The first systematic study of the phenomenon was published in 1906 by Lobsien,[16] but it was not until Ballard's investigation in 1913[17] that much notice was taken of these results as in disagreement with the classical retention-curve of Ebbinghaus. Ballard's study was followed by those of Huguenin,[18] Nicolai,[19] Brown,[20] and Williams.[21]

In spite of its apparent frequency of occurrence, reminiscence has received comparatively little attention by students in the field of learning. There are probably two reasons, at least, for this neglect, the first one of which is Brown's study of the extent to which memory is measured by a single recall. He found that "a second recall may contain more items than the first because repetition tends to fix the items of the first recall while chance tends to introduce new items into the second recall in addition to those retained from the first recall."[22] Since, however, the factors of repetition and chance are free to operate in all experiments on retention, it is significant that improvement on the second recall is not universally found. The problem resolves itself into the question: under what conditions is the typical Ebbinghaus retention-curve obtained and under what conditions does the curve approximate that found by Ballard?

The second reason for the neglect of reminiscence is the fact that it has not yet been demonstrated to occur independently of review, either intentional rehearsal or casual revival, during the interval between the two tests of retention.

By questioning many of his juvenile *Ss*, Ballard discovered that from 20–30% in some classes had 'thought about' the poetry in the interval but that generally seldom over 5% had reviewed.[23] The results obtained from such questioning[24] were neither consistent nor statistically treated. He concluded, however, that the "subjects who casually think of the passage of poetry during the interval between the two tests show slightly more reminiscence than those who do not."[25] Furthermore, on the basis of one experiment in which the *Ss* were instructed to review, but were put on their honor not to talk to each other or look at the poem, during an interval of 3 days, he suggested that "reminiscence is influenced, though to no very great extent, by repeated efforts to recall."[26]

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[16] M. Lobsien, Über das Gedächtnis für bildlich dargestellte Dinge in seiner Abhängigkeit von der Zwischenzeit, *Beitr. z. Psychol. d. Aussage*, 2, 1905, 17-30.

[17] P. B. Ballard, Obliviscence and reminiscence, *Brit. J. Psychol., Monog. Suppl.*, 1, 1913, (no. 11), 1-82.

[18] C. Huguenin, Reviviscence paradoxale, *Arch. d. Psychol.*, 14, 1914, 379-383.

[19] Friedrich Nicolai, Experimentelle Untersuchungen über das Haften von Gesichtseindrücken und dessen zeitlichen Verlauf, *Arch. f. d. ges. Psychol.*, 42, 1922, 132-149.

[20] Warner Brown, To what extent is memory measured by a single recall?, *J. Exper. Psychol.*, 6, 1923, 377-382.

[21] Osborne Williams, A study of the phenomenon of reminiscence, *J. Exper. Psychol.*, 9, 1926, 368-387.

[22] Brown, *op. cit.*, 382.

[23] Ballard, *op. cit.*, 39.

[24] *Ibid.*, 33.

[25] *Ibid.*, 80.

[26] *Ibid.*, 34.

The effect of casual revival, as well as of intentional rehearsal, is of basic importance with respect to the validity of reminiscence since, until its independence of review is established, it can be considered a special case of distributed practice, and consequently not a unique phenomenon accompanying the condition of disuse. An attempt to test the effect of review upon reminiscence[27] was made in two experiments described in the following pages.

Experiment I

The purpose here was to study the effect of review upon the reminiscence obtained by repeating those conditions of Williams' experiment which had been optimal in yielding the phenomenon, viz., third and fourth grade school children, poetry, and an interval of 24 hrs.

The *Ss* (403) were all the third and fourth grade school-children in five public schools of Columbia, Mo. The material used was the same in form and content as that employed by Williams; the first 24 lines of "The Spider and the Fly" by Mary Howitt. Each child was given a copy of the poem printed on thin cardboard. No instructions were given concerning the method of learning. There was an interval of 24 hrs. between the recall taken immediately after the 5-min. learning-period and the delayed recall, which will be designated the retention-test. Williams' method and procedure of conducting the experiment described in his article were duplicated in every detail. After all the *Ss* had finished the retention-test they were asked:

(1) Whether they had ever heard or read the poem before.
(2) If so, had they learned it or any part of it.
(3) Had they read or seen the poem during the 24-hr. interval.
(4) Had they talked or thought about the material during the interval.

When answering the first two queries, those children who declared that they had previously learned the poem were questioned individually by *E* who noted the circumstances of their learning. The last question was explained very carefully and the children were encouraged and given opportunity to relate the conditions under which they had reviewed the material. They were told even if only a line or part of a line had spontaneously popped into their heads, or if they had heard anyone reciting the poem, they should answer Question 4 affirmatively. *E* tried to make the whole discussion suggest that she both expected and approved of rehearsal so that considerable reliability could be placed on the statements of those *Ss* who maintained, in spite of contrary suggestions, that they had not reviewed.

All scoring was done by the writer, who employed Williams' method. Two

[27] Ballard differentiated between improvement and reminiscence. Improvement consisted of a larger total score on the second recall than on the first recall either for the individual or for the group, whereas reminiscence referred to those specific items which did not appear in the first recall but did appear in the second. With Ballard's terminology, there could be reminiscence without improvement, but not vice versa. The term reminiscence has been used rather loosely in the literature to refer to both conditions. In this paper we are concerned only with the more unusual phenomenon of improvement in the total score and no account will be taken of the specific items which constitute the total score. The usage employed by Williams has been adopted: improvement and reminiscence are used synonymously.

types of scores were obtained; V-scores, which are the number of lines reproduced *verbatim*, and S-scores, which are the V-scores added to the number of lines reproduced as to sense.[28] Williams' third type of score including partial sense was omitted, since its reliability was questioned as a result of the discrepancies obtained by marking a sampling of cases twice.

Results. Of the 403 children tested, the records of only the 295 (73%) who had neither heard nor seen the poem during the interval were used. These *S*s were divided into two groups on the basis of their answers to Question 4; a Review Group of 247 (84%) who had acknowledged rehearsal, and a No-Review Group of 48 (16%) who had denied any reviewing. Reminiscence was measured in the following three ways: (1) the percentage of retention based on the average scores of the groups; (2) the percentage of subjects showing reminiscence; and (3) the average number of lines of reminiscence obtained from those subjects who improved. These three measures for both V- and S-scores are summarized in Table I.

TABLE I

REVIEW *vs.* NO-REVIEW

(A) Mean scores

		V-Scores					S-Scores				
		Learning		Retention		% of R	Learning		Retention		% of R
Group	N	Mean	σM	Mean	σM		Mean	σM	Mean	σM	
Review	247	2.22	.11	2.21	.12	99.5	6.38	.21	6.59	.22	103.2
No-Rev.	48	2.27	.29	2.39	.33	105.2	6.22	.65	6.20	.69	99.6
D and σD		−.05	.31	−.18	.35	−5.7	.16	.68	.39	.72	3.6

(B) Reminiscence shown

	V-Scores					S-Scores				
	Ss Showing Rem.			Amount		Ss Showing Rem.			Amount	
Group	N	%	σP*	Mean	σM	N	%	σP*	Mean	σM
Review	60	24.2	2.7	1.62	.10	90	36.4	3.1	2.27	.17
No-Rev.	13	27.0	6.4	1.76	.26	14	29.1	6.5	2.92	.38
D and σD		−2.8	6.9	− .14	.28		7.3	7.2	−.65	.42

* Sigma of a proportion. See J. K. Holzinger, *Statistical Methods for Students in Education*, 1928, 248.

Inspection of this table reveals that the small differences between the Review and No-Review groups in percentages of retention are not reliable

[28] The S-scores were not treated separately because our interest is in the total number of lines learned and recalled. The total number of lines can be measured by the more severe criterion of *verbatim* reproduction or by the more lenient and inclusive criterion of sense-reproduction.

since the differences between the two groups in mean retention-scores are not statistically valid. Similar results are obtained with the percentages of *Ss* showing reminiscence and with the mean number of lines of reminiscence shown, *i.e.* in no comparison between the two groups is the difference reliable. It is interesting to note that in 4 out of 6 comparisons (3 measures of reminiscence with 2 types of scores) the differences obtained, although unreliable, are in the direction of a greater degree of reminiscence with the No-Review Group. These rather unexpected results justify the following conclusion. Under the conditions of this experiment reminiscence is independent of the factor of review since there are no reliable differences with either V- or S-scores in any of the three ways of measuring reminiscence between the group of *Ss* who admitted reviewing and those who denied it.

The significance of these results depends upon the accuracy of our procedure of separating the *Ss* upon the basis of the revival during the interval. Within the limits of our experiment there was no way of measuring how accurately the children reported or how accurately they could report. Probably the fact that such a small number (48 or 16%) denied any reviewing argues for the validity of their assertions, since, by chance alone, one would expect at least some of the *Ss* to be free from rehearsal. The small number of cases, however, works against the statistical validity of the results.

Experiment II

The procedure of questioning the children concerning review used in the first experiment was repeated in Experiment II, which was also designed to test the effect upon reminiscence of the method of learning and of intelligence.

The *Ss* (440) were all the 9-, 10-, and 11-yr.-old children in six public schools, four in Fayetteville, and two in Springdale, Arkansas. The same poem was used but a different section of it and in a different form. Stanzas 4-9 (24 lines) were arranged in 12 lines to facilitate the mechanical requirements of learning by the part-method. Since in scoring, each line was treated as two, the scores are comparable to those in Experiment I.

Half of the children learned by the whole-method and the other half by a pure part-method. For the whole-method, the 12 lines were printed single-space on sheets of paper. The *Ss* were told that they had 5 min. in which to learn the poem by reading all the way through it as many times as they could. As in Experiment I and in that of Williams, the learning period of 5 min. was preceded by *E*'s reading the material aloud once to the class. The children who used the part-method were given the material printed with 1 in. between lines. Each line was first read aloud once by *E* and then 20 sec. were allotted for study before the next line was read. A blank sheet of paper used to cover the poem was slipped down exposing each line in turn until all 12 lines had been studied. The last minute of the 5 given to learning was devoted to a review of the whole poem. Before the experiment began the children were given practice in slipping the blank

sheet down line by line on the back of the paper on which the poem was printed so that no time would be lost in this manipulation during the learning period.[29] As before, *E* aimed to give the impression that the experiment was over when she collected the papers on which the immediate recalls were written. The retention-

TABLE II

REVIEW *vs.* NO-REVIEW

(A) Mean scores

Whole-Method

		V-Scores					S-Scores				
		Learning		Retention		% of R	Learning		Retention		% of R
Group	N	Mean	σM	Mean	σM		Mean	σM	Mean	σM	
Review	110	1.95	.22	1.54	.18	78.9	8.19	.41	8.00	.40	97.6
No-Rev.	51	2.23	.36	2.05	.35	91.9	9.58	.64	8.96	.65	93.5
D and σD		−.28	.42	−.51	.39	−13.0	−1.39	.76	−.96	.76	4.1

Part-Method

Review	110	2.50	.22	2.20	.27	88.0	8.55	.42	7.45	.43	87.1
No-Rev.	39	2.51	.34	1.89	.44	75.3	8.66	.64	7.59	.65	87.6
D and σD		−.01	.40	.31	.51	12.7	−.11	.76	−.14	.77	−.5

(B) Reminiscence shown

		V-Scores			S-Scores			
	Group	N	% showing	Mean Amount	N	% showing	Mean Amount	σM
Whole-Method	Review	27	24.5	1.40	43	39.0	2.46	.25
	No-Rev.	12	23.5	1.41	14	27.4	1.64	.21
	D		1.0	−.01		11.6	.82	
Part-Method	Review	22	20.0	1.45	22	20.0	2.31	
	No-Rev.	6	15.4	2.00	9	23.1	2.55	
	D		4.6	−.55		−3.1	−.24	

test occurred 24 hr. later, after which the four questions described above were answered. Several days after the experiment, the National Intelligence Tests, Scale A, Form I, were administered by the writer.

Results. The records of only the 310 (70%) children who had not heard or learned the poem before or seen it during the 24-hr. interval were used. Of the 161 who learned by the whole-method, 110 (68%) admitted reviewing and 51 (32%) denied it; of the 149 children who used the part-method, 110 (74%) were in the Review Group and 39 (26%) in the No-Review Group. A study of the data summarized in Table II shows

[29] For a more detailed description of the procedure see G. O. McGeoch, The whole-part problem in memorizing poetry, *Ped. Sem.*, 43, 1933, 439-447.

that no reminiscence was obtained with either group when improvement was measured in terms of percentages of retention based on the learning- and retention-scores of the group. The inadequacy of the percentage of retention of the group as a measure of reminiscence will be discussed in a later section (Method of Measuring Reminiscence). There are no reliable differences between the Review and No-Review groups with either whole- or part-method of learning in the percentages of retention since the differences in mean scores in learning and in retention are not valid.

The data from the two other measures of reminiscence, the percentage of each group exhibiting it and the mean number of lines shown, are also given in Table II. It is unfortunate that the small number of cases in the No-Review Group precludes a statistical treatment of the differences between the two groups, with the possible exception of the S-scores with the whole-method. Here a difference of 11.6 in the percentages of *Ss* showing reminiscence in favor of the Review-Group is not valid since the sigma of the difference is 7.7. In the amount of reminiscence shown, the difference of 0.82 of a line in favor of the Review-Group has a sigma of 0.32 which gives a critical ratio of 2.56. A tendency for the Review-Group to exhibit more lines of reminiscence is not, however, apparent in any of the three other comparisons; viz., whole-method with V-scores and part-method with V- and S-scores. In fact, the differences obtained are in favor of the No-Review Group. The data justify the conclusion that there is no consistent relationship between reminiscence and rehearsal. To this extent the results of Experiment II corroborate those of Experiment I.

Attention should be called to the large percentage of *Ss* (84% in Experiment I and 71% in Experiment II)[30] who reported reviewing even though there was no reason to expect a second recall. Under such 'natural conditions' of classroom experimentation the factor of review is very real but difficult of control. An experimental technique more adequate than ours would be to isolate the review-factor by preventing its occurrence. This procedure was attempted in a previous study on the whole-part problem[31] in which each *S* promised *E* that he would not talk or think about the material during the 24-hr. interval and reported later how well he had been able to carry out his promise. Under these conditions, in which the writer believes that very little rehearsal occurred, the following results were obtained: 4% of the records showed reminiscence with difficult vocabulary material (Experiment 3);[32] 9% with less difficult vocabulary material (Experiment 1); 13% with one poem (Experiment 2); and 21% with another (Experiment 4). This last percentage approximates the percentages obtained in the two experiments here reported.

[30] The discrepancy between these percentages and the usual 5% reported by Ballard (p. 39) which was referred to previously is worth noting. The explanation may lie outside of the difference in the *actual* number of subjects who reviewed and in the technique of questioning, particularly in the emphasis and suggestion used. It is significant that in our experiments any reminiscence was obtained with the small percentage who denied rehearsal.

[31] McGeoch, The intelligence quotient as a factor in the whole-part problem, *J. Exper. Psychol.*, 14, 1931, 333-358.

[32] The numbers of the experiments refer to those of the paper cited in footnote 31.

On the basis of the results of the two present experiments, substantiated by the results obtained in a previous study, it is concluded that improvement in memory after an interval of time is not necessarily a function of the adventitious factor of review. Reminiscence cannot therefore be considered a special case of distributed practice, but it lays a claim on attention as an independent and valid phenomenon accompanying the factor of disuse. In the light of such validity, a critical analysis of the available experimental data on reminiscence is in order.

Comparison of Experiment I with Williams' experiment. Such an analysis may readily begin with a comparison of the results of Williams' experiment with his youngest age-group and a 24-hr. interval with the results from our Experiment I. Both used the same experimental conditions. Since Williams "discarded the records of those *Ss* who had previously learned this poem or who admitted that they read it during the interval,"[33] he apparently did not take into account the children who had not *learned* the poem previously but who had *heard* it before, *i.e.* to whom the material was not new. The most valid comparison, therefore, is between Williams' data and our results with the 339 *Ss* who had not previously learned the material. To 295 of these children the poem was new, while to 44 it was familiar (see section on Familiarity of Material). Inspection of Table III in which the data are summarized yields the following differences.

TABLE III

EXPERIMENT I AND WILLIAMS' EXPERIMENT

					Learning		Retention		%
		N	Age	$\sigma_{dis.}$	Mean	$\sigma_{dis.}$	Mean	$\sigma_{dis.}$	R
V-Scores	Exper. I	339	9.61	1.26	2.34	1.93	2.36	2.23	100.85
	Williams	106	10.27		6.22		7.10		114.15
S-Scores	Exper. I				6.55	3.75	6.76	4.00	103.20
	Williams				14.59		15.56		106.65

(1) Williams' *Ss*, Chicago public-school children of the third and fourth grades, learned and retained more than twice as much in terms of both V- and S-scores as our *Ss*, public-school children of the same grades in Columbia, Mo. Since Williams published no measures of variability, neither the reliability of these differences in average scores nor the reliability of the difference in average age can be determined. Even assuming, however, that the difference in age of about 8-mo. in favor of the Chicago children is reliable, it is not probable that their superior performance is due entirely to the age-factor. Other possible explanations of the

[33] Williams, *op. cit.*, 372. The data quoted here are from 377, Table V, "One-day interval."

large differences between the two groups are (1) the Chicago children were as a group more intelligent, (2) they had had more practice in memorizing, (3) there were some unintentional or accidental variations in the experimental procedures or conditions, and (4) the two experiments differed in the method of scoring. As soon as the great discrepancy in results between the two experiments became apparent, the writer marked every paper twice with an interval of at least one day between markings. With the *verbatim* scores, at least, which are highly objective, it is difficult to conceive how such great differences could be attributable to scoring procedure. From the published data there is no way of determining the effect of the factors suggested above.

(2) The superiority, in Williams' data, of the V-scores over the S-scores in percentage of retention and consequently in reminiscence was reversed in our experiment. This reversal may be a function of the differences between the Chicago and Columbia children in the amount of previous training in rote learning.

(3) With both types of score, Williams' data show a greater amount of reminiscence as measured by the percentage of retention than ours. Here again, it is unfortunate that the reliability of these differences cannot be computed. Differences between the two experiments which are based upon the percentages of retention of the groups are, however, of minor significance with respect to an analysis of reminiscence as the discussion in the following section will demonstrate.

Method of measuring reminiscence. Williams published only one measure of reminiscence, the percentage of retention of the group. Ballard had used this measure as well as the percentage of improvement or of loss, also derived from group-learning and retention-scores. He published in addition, however, the percentage of *S*s who exhibited reminiscence irrespective of the amount. Do these measures yield similar results and are they equally valid? Comparisons can readily be made from the data in several of Ballard's tables.

In an extensive experiment with "The Ancient Mariner," the percentage of loss for intervals of 1, 4, 5, 6, and 7 days was 1.6, 2.0, 5.8, 8.7, and 12.1 respectively, yet the percentages of children who improved were, respectively, 45.5, 40.6, 42.2, 30.0, and 28.4. With "The Wreck of the Hesperus," 44.6% and 31.7% of the children remembered more after intervals of 6 and 7 days, yet the table shows a loss in percentage of retention for these two intervals. Again, with nonsense verse, although for 5 intervals there was a loss of from 8.0% to 27.7%, nevertheless from 5.6% to 15.9% of the children yielded reminiscence. With adults the picture is the same: although from 14.3% to 41.0% of the 5 groups improved after various intervals, yet forgetting (not reminiscence) is manifested in the percentage of retention for each group. The phenomenon of reminiscence is of all-or-none character with respect to individual *S*s. They either remember more on the delayed recall than on the immediate recall or they do not. In the above data, however, the masking caused by using group-averages actually conceals the presence of reminiscence. One cannot tell from the percentages of retention of the group whether any reminiscence occurred or not, and conversely, one cannot tell from the percentages of *S*s showing reminiscence whether the group retention-score will

indicate improvement or loss. It is quite obvious that the percentage of *Ss* exhibiting reminiscence is a more valid measure of reminiscence itself than percentages derived from group-scores. Another example of the inadequacy of the percentage of retention of the group as a measure of reminiscence will be found in the section on Degree of Learning.

There is a further limitation to this measure besides that derived from its calculation on the basis of group-averages. This is most clearly apparent in Ballard's experiments on the age-factor from which he concluded that with age, relative reminiscence (percentage of improvement) decreases, while absolute reminiscence (the number of lines recovered) increases. Since the percentage of improvement is the ratio between the number of lines recovered and the number of lines learned initially, it is apparent that the larger the learning-score the greater will have to be the number of lines of improvement to maintain a given ratio. Thus, under most conditions, the chances of a higher percentage of improvement are weighted in favor of the *Ss* with the smaller learning scores. Overlooking even the total lack of uniformity in experimental conditions with different age-levels, Ballard's age comparisons are invalidated by the fact that there is a tendency for the average number of lines learned by the younger age-groups to be smaller than those learned by the older *Ss*. For instance, the older children learned 14 out of 34 lines in one experiment and 15 out of 72 lines in another, whereas the *Ss* from the infant schools learned in two experiments 5.5 lines and 3.1 lines out of presumably 10 or 12 lines. Since the percentage of improvement here is an equivocal measure of reminiscence, Ballard's results in terms of the number of lines recovered should receive more serious attention.

It will have been observed that in the two experiments reported in this paper three measures of reminiscence are given, viz., the percentage of *Ss* showing reminiscence, the average number of lines obtained from those who improved, and, for purposes of comparison with other studies, the percentage of retention of the group.

Involved in the problem of measuring reminiscence is the implied problem of the various methods of scoring the data, *i.e.* the criterion of both learning and retention. It has been seen above that reminiscence varies with *verbatim* scoring of poetry compared to sense scoring. Ballard used the facility or speed of written reproduction of poetry, and concluded that a larger percentage of children showed reminiscence measured in terms of facility of recall than in terms of gain in the amount recalled. In view of the inadequacy of his data, however, this conclusion can be considered only suggestive. The methods of scoring the data are, of course, determined by the type of material and the experimental technique, but, with the same type of material, minor differences in scoring should not be overlooked when comparing results of different experiments.

Our analysis has revealed that the percentage of *Ss* exhibiting reminiscence and the amount shown are valid measures of reminiscence, whereas measures derived from learning- and retention-scores of the whole group are inadequate and, under some conditions, the percentage of improvement is an equivocal measure.

Nature of the material. Two main questions concern the effect of the nature of the learning material. (1) Are all materials susceptible to reminis-

cence? and (2) Which material is most susceptible? A review of the experimental literature gives an incomplete answer to the first question.

Under widely diverse experimental conditions the following materials have yielded reminiscence; poetry (Ballard, Huguenin, Williams, McGeoch); prose (Henderson, Colvin and Myers, Ballard, Guillet, King and Homan, Gordon); nonsense syllables (Ballard, Gates, Guillet); paired associates (Ballard, McGeoch[34]); lists of unrelated words (Guillet, Myers,[35] Brown); lists of related words (Guillet); incidental memory of words (Myers[36]); figures (Guillet); and material observed under *aussage* technique (Lobsien, Nicolai, Winch,[37] McGeoch[38]). It is evident from the discussion on the method of measuring reminiscence that a large percentage of a group could yield reminiscence without such improvement being apparent in the group retention-score. In those experiments, therefore, such as Ballard's with figures and Williams' with poetry and lists of unrelated words in which only the average scores of the group are given, a percentage of retention not over 100 is not valid evidence of the absence of reminiscence. In fact, it is highly probable that in many studies of retention in which the Ebbinghaus type of curve was obtained in terms of average scores reminiscence would become evident if the data were analyzed in terms of the percentage of *Ss* showing improvement. Such a re-analysis of results might yield evidence of reminiscence also in the retention of various acts of skill, studies of which are lacking in the materials listed above.

The present available experimental data do not furnish even a tentative answer to the second question as to which material is most susceptible to reminiscence. The widely varied experimental conditions in the different studies do not permit comparisons of results. The percentage of *Ss* showing reminiscence would be the most valid measure possible in comparisons of results with different types of materials. Until the effect of the degree of learning upon reminiscence is established, however, the equating of such factors as amount of material, length of learning time, etc., is uncertain and complicated. The lack of uniformity in length of poem and time devoted to learning in Ballard's three extensive experiments with ballad poetry and nonsense verse and in other conditions throughout his study renders unreliable his general conclusion that the more comprehensible and interesting poems to children will yield greater amounts of improvement. Likewise, Guillet's conclusion that the phenomenon "was proportionate to the meaningfulness of the material,"[39] awaits experimental verification.

Analysis of the literature has shown that many types of material are susceptible to reminiscence and has suggested that the phenomenon is more prevalent than extensive data on retention in their present form would reveal. The question as to the material most susceptible to reminiscence awaits further experimentation.

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[34] McGeoch, *op. cit., J. Exper. Psychol.*, 14, 1931, 333-358.
[35] Myers, *op. cit., J. Educ. Psychol.*, 8, 1917, 166-175.
[36] Myers, *opp. citt., Arch. Psychol.*, 4, 1913, (no. 26), 1-108; *J. Educ. Psychol.*, 5, 1914, 119-130.
[37] Winch, *op. cit., Educ. Psychol., Monog.*, 1914, (no. 12), 1-245.
[38] McGeoch, *op. cit.*, M.A. thesis, 47. [39] Guillet, *op. cit.*, 75.

Degree of learning. With respect to the degree of learning, Gordon's conclusion[40] that reminiscence occurred more frequently among poorer learners can be considered only suggestive in view of her small number of cases of reminiscence, 11 out of 297 *Ss*.

Williams made a study of the relationship between degree of learning and reminiscence by dividing his *Ss* into three groups on the basis of the number of lines learned. With V-scores, he obtained the following percentages of retention (which are based on the five time intervals which he used): lowest third 142.81%, middle third 105.23%, and highest third 103.22%. He concluded that "within a given age-group the greatest amount of reminiscence is exhibited by the poorest learners."[41]

The data of our Experiment I on the 295 children to whom the poetry was new were similarly analyzed on the basis of the amount learned. With the V-scores, all the *Ss* who had scores of 1 or 2 were put in the Lowest Group, those with scores of 2 or 3 were put in the Middle Group, while those with scores of 4 or over constituted the Highest Group. With the S-scores, the basis of division into the three groups was as follows: scores from 0 through 4, scores of 5 through 7, and scores of 8 or over. The results are summarized in Table IV.

TABLE IV

DEGREE OF LEARNING: EXPERIMENT I

	Group	N	% of retention	Ss showing remin. %	Ss showing remin. σp	Amount Mean	Amount σM	Differences in Amount Group	Differences in Amount Diff.	σD
V-Scores	Lowest	120	127.27	25.8	4.0	1.22	.07	H—L	.89	.24
	Middle	112	98.47	21.4	3.8	1.83	.21	H—M	.28	.31
	Highest	63	95.92	28.5	5.7	2.11	.23	M—L	.61	.22
S-Scores	Lowest	111	124.92	36.0	4.6	2.77	.34	H—L	−.43	.45
	Middle	90	98.30	33.3	5.0	1.86	.21	H—M	.48	.36
	Highest	94	97.51	34.0	4.9	2.34	.30	M—L	−.91	.40

Although a direct comparison of Williams' results with those of our Experiment I cannot be made, since, as was mentioned above, Williams' figures are averages of five different intervals, nevertheless it should be noted that when reminiscence is measured by percentage of retention there is agreement between the two experiments in that the poorest learners show the most reminiscence. An analysis of the data of our Experiment II revealed the same relationship with both whole- and part-methods. Passing over the question of the reliability of these consistent differences between percentages of retention, we turn to the two more valid measures of reminiscence. Considering first the percentages of children showing reminiscence in Table IV, one notices the small differences between the three groups with both types of score. The sigmas of these differences were computed and found to be about as large as, if not larger than, the obtained differences. There is,

[40] Gordon, *op. cit.*, 343.

[41] Williams, *op. cit.*, 376.

therefore, no doubt as to their unreliability. The data of Experiment II yielded the same results, *i.e.* no reliable differences with either whole- or part-method of learning between the Lowest, Middle, or Highest Group in percentages of *Ss* showing improvement.

Turning to the average number of lines of reminiscence obtained which is given in Table IV, one finds with the V-scores a consistent tendency for the better learners to yield more reminiscence, but no such tendency with the S-scores. The consistency of the tendency noted with the V-scores is given statistical support by the fact that the difference between the Highest and Lowest Group is reliable, while the chances are 997 out of 1000 that the direction of the difference between the Middle and Lowest Group is correct. Although the difference between the Middle and Highest Group is not valid, it can be concluded that the poorest learners, the Lowest Group, show fewer number of lines of reminiscence in V-scores than the better learners, the Middle and Highest Groups. This conclusion is in direct opposition to Williams' generalization based on percentages of retention of the group. Analysis of the data of our Experiment II resulted in finding no reliable differences with either whole- or part-method between the three groups with either type of score in the average number of lines of reminiscence obtained. In connection with these negative results, however, it should be noted that the small number of cases in some of the groups precluded the possibility of statistical treatment.

Another example of the inadequacy of the percentage of retention of the group as a measure of reminiscence is seen in Table IV. On the basis of the percentages of retention, the Middle and Highest Groups show, not reminiscence, but forgetting, whereas, in fact, from one-fifth to one-third of the children remembered more on the second recall than on the first. It is therefore justifiable, in analyzing the degree of learning as a factor in reminiscence, to use those results in Experiments I and II which are based upon the percentages of *Ss* yielding improvement and the average number of lines of reminiscence obtained and not on results in terms of the percentage of retention of the group. These results are as follows: (1) there is no reliable tendency for the percentage of *Ss* showing reminiscence to vary with the degree of learning in either experiment; (2) there is no reliable variation with the degree of learning in the number of lines of reminiscence obtained with the S-scores in both experiments and with the V-scores in Experiment II; (3) with the V-scores in Experiment I there is a reliable tendency for the better learners, Highest and Middle Groups, to show a greater number of lines of reminiscence than the poorest learners, Lowest Group.

On the basis of the present published data, the precise relationship between the factor of degree of learning and the phenomenon of reminiscence cannot be stated.

Sex differences. It is interesting to note that Williams' conclusion that the poorest learners showed the most reminiscence was followed by an analysis of sex differences in which he concluded that "the girls exhibited a greater degree of reminiscence"[42] although the girls were consistently

[42] Williams, *op. cit., J. Exper. Psychol.*, 9, 1926, 384.

superior to the boys in both learning and retention. His data on sex differences, including the mean V- and S-scores in learning and retention and the percentages of retention for the two sexes, are given in Table V below.

TABLE V
SEX DIFFERENCES

		V-Scores					S-Scores				
		Learning		Retention		% of	Learning		Retention		% of
Sex	N	Mean	σM	Mean	σM	R	Mean	σM	Mean	σM	R
Experiment I											
Girls	157	2.26	.14	2.43	.17	107.52	6.43	.27	6.98	.32	108.55
Boys	138	2.12	.16	2.03	.17	95.75	6.27	.33	6.01	.29	95.85
D and σD		.14	.21	.40	.24	11.77	.16	.43	.97	.43	12.70
Williams											
Girls	57	6.48		8.04		124.07	15.02		16.35		108.85
Boys	49	5.49		6.02		109.65	14.10		14.63		103.75
D		.99		2.02		14.42	.92		1.72		5.10

Overlooking the fact that he drew his conclusions from results based on an inadequate measure of reminiscence, one wonders if the obtained differences between the percentages of retention are reliable.

Since no measures of variability of his mean learning and retention scores are published, it is impossible to compute the reliability of any differences. A statistical analysis of the data of Experiment I of the present paper was made, the results of which, including the V- and S-scores for learning and retention with the sigmas of the means, the percentages of retention, the differences between mean scores and the sigmas of these differences, are also presented in Table V. Although the results agree with those of Williams in that there are consistent differences in favor of the girls, it will be noted that none of the differences between the sexes, in either the mean learning-scores or in the mean retention-scores, is reliable. It follows, therefore, that the differences between the percentages of retention cannot be valid. A further analysis of the data of Experiment I on the basis of the two more adequate measures of reminiscence yielded the following results: reminiscence was shown by 23.6% ± 3.4% of the girls compared to 24.6% ± 3.6% of the boys with the V-scores, and 39.5% ± 3.9% of the girls compared to 28.2% ± 3.8% of the boys with the S-scores. Neither of these differences is reliable, nor are the differences, which favor the girls, in the average number of lines of reminiscence obtained, since, with the V-scores, the difference is 0.47 with a sigma of 0.20 and with the S-scores, 0.40 with a sigma of 0.32. This lack of valid sex differences in reminiscence in Experiment I is corroborated by a similar analysis of the data of Experiment II, which revealed no reliable differences between the sexes in percentages of retention, percentages of subjects improving, or the average number of lines of reminiscence obtained.

Ballard concluded on the basis of an incidental analysis of some of his data

that there were no significant sex differences in the amount of improvement shown. The writer treated statistically the data in Myers' experiment and found that there were no reliable differences between the sexes in the percentages of *Ss* exhibiting reminiscence.

In the light of the above evidence it appears that Williams' conclusion is probably invalid. Sex has not been demonstrated to be a factor in reminiscence.

Intelligence. The factor of the intelligence of the learner, which is related to the factor of degree of learning, was not dealt with by Williams. Ballard studied the relationship between general intelligence and reminiscence in experiments on feeble-minded children, normal children, and women students in a training college. From the results on the first group he concluded that reminiscence "varies inversely with the extent of the subject's general mental equipment."[43]

The results on the third group tended to confirm this hypothesis, but the ambiguous results from the more extensive investigations on normal children led him to emphasize the complexity of the relationship between reminiscence and general ability, and to suggest that the age factor was more fundamental than intelligence (see section on Age). Ballard concluded, nevertheless, that "generally speaking, among children of the same age, the more intelligent learn faster, retain better, and recover more than the less intelligent. The latter, however, show a higher percentage of recovery or reminiscence."[44] His results do not warrant such a general statement concerning the relationship between reminiscence and mental ability for the following reasons: (1) throughout the different levels of intelligence his experimental conditions were not constant; (2) his measures of general ability were neither constant nor adequate; (3) the small size of his groups makes statistical reliability improbable; and (4) his data are mainly in terms of the number of lines recovered (reminiscence according to his usage) and not improvement (reminiscence according to our usage).

It will be remembered that in Experiment II all of the 9-, 10-, and 11-yr.-old children in each school studied were used so as to obtain a wide range of intelligence. For the three ages, respectively, there were 73, 112, and 115 *Ss* whose range in the National Intelligence Test scores was 13-121, 31-139, and 36-167. The data were first analyzed by correlating within each age-group the intelligence test-score of each *S* with his percentage of retention. These correlations, which ranged between $-0.02 \pm .06$ and $-0.26 \pm .09$, showed no definite tendency for reminiscence to vary with intelligence. There were, however, 30% of the *Ss* who yielded reminiscence but whose learning scores of zeros made computation of the percentages of retention for these children impossible. It is apparent that such a correlational treatment of inadequate measures of reminiscence throws little light upon the relationship between reminiscence and mental ability.

The *Ss* were then divided into three groups, within each age level, on the basis of their National Intelligence Test scores. The lowest third in intelligence was

[43] Ballard, *op. cit.*, 43. [44] *Ibid.*, 81.

compared with the highest third in terms of the percentages of *Ss* showing reminiscence and in the average number of lines exhibited. Working over the data with the whole-and part-methods separated revealed no differential effect of learning-method upon results. Only the results, therefore, with the two learning-methods combined are presented in Table VI where will be found for the highest and lowest thirds of each age the average intelligence scores, the range, the number of *Ss* showing reminiscence, the percentages with their sigmas, the differences in percentages with the sigmas of the differences, and the average number of lines of improvement obtained. Unfortunately, the small absolute number of cases yielding reminiscence precludes any generalization on the basis of these results, but it is evident from a study of Table VI that the differences between the two intelligence-groups in percentages of *Ss* showing reminiscence are, without exception, too small to be reliable.

TABLE VI

INTELLIGENCE

Age	Thirds	N	Intel.-Scores		Ss showing remin.						Mean No. of lines	
					V-Scores			S-Scores				
			Mean	Range	N	%	σP	N	%	σP	V	S
	Highest	25	93.2	70–121	8	32.0	9.3	10	40.0	9.8	1.50	2.50
9	Lowest	25	42.2	13–59	3	12.0	6.5	7	28.0	9.0	1.00	1.57
	D and σD					20.0	11.3		12.0	13.3	.50	.93
	Highest	38	111.2	98–139	11	28.9	7.4	16	42.1	8.0	1.36	1.93
10	Lowest	39	56.6	31–72	9	23.0	6.8	9	23.0	6.8	1.33	2.11
	D and σD					5.9	10.0		19.1	10.5	.03	−.18
	Highest	34	124.8	109–167	5	14.7	6.0	9	26.4	7.5	1.80	2.77
11	Lowest	35	61.5	36–84	7	20.0	6.8	11	31.4	7.8	2.28	3.18
	D and σD					−5.3	9.0		−5.0	10.8	−.48	−.41

Furthermore, although the number of cases is too small for statistical treatment, the results in terms of the amount of reminiscence obtained show no marked or consistent tendency for the lowest thirds to yield more improvement than the highest thirds. The differences between the two intelligence groups is apparently affected by age, but the cases are too few in number to justify such a conclusion. Since Ballard did not publish the percentages of *Ss* showing reminiscence (improvement) or the average number of lines shown, comparisons between his results and ours cannot be made. Neither his data, however, nor ours justify the conclusion that intelligence is a factor in the phenomenon of reminiscence.

Familiarity of material. Reminiscence has been defined in this paper as an improvement in the recall of incompletely learned material after an interval of time without intervenient formal review or relearning. Material which has once been learned to complete mastery, therefore, cannot yield reminiscence, even though there is an improvement in recall after an interval during which no systematic review or relearning occurred. A comparison

of the retention of material which has previously been learned to complete mastery with the retention of entirely new material after both materials have been subjected to the same experimental procedure of a formal learning-period, followed by a test of recall after a given interval, provides a possible setting for a demonstration of Jost's Law, but not for an analysis of the effect of the age or familiarity of the material upon the phenomenon of reminiscence.

Ballard's conclusion concerning age of learning material is an instance of the above fallacy. On the basis of published results of three experiments and corroborative data from similar tests he concluded that greater reminiscence is found with old memory-material. The poems used in his second and third experiments, however, had been learned, presumably, to complete mastery four years previously. The poem in his first experiment had not been memorized before, but was quite familiar to the *Ss* since it had been in a text used the previous year. This poem yielded an average improvement of 40% on the delayed recall which occurred the day following the learning period. Ballard is not justified in concluding from these results that the familiarity of the material accounts for, what he considered, an unusual amount of improvement, since no control experiment was run in which the same poem was entirely new to the *Ss*.

The two learning materials employed by Brown under roughly comparable experimental conditions would furnish an opportunity for studying the effect of age of material upon reminiscence if it were not for the fact that the familiar material, names of the 48 states, had, probably, at one time been learned to complete mastery by the majority of his adult *Ss*. It is, however, interesting to note in passing that after a 30-min. interval there was a gain of 3.35 items with the familiar material compared to a gain of 1.29 items with the new material, 48 unconnected common words from the key list of the Berrol memory-system. On the basis of the published data the reliability of this difference of 1.06 items cannot be computed.

The factor of familiarity or age of the material was studied in Experiment I. On the basis of the answers to Questions 1 and 2 the *Ss* were divided into the two following groups: the Familiar Group, consisting of the 44 children who had heard or read the poem before but had not memorized any part of it; and the Unfamiliar Group, consisting of the 295 children to whom the poem was entirely new. Examination of the upper section of Table VII reveals evidence in support of the reliability of the grouping. The Familiar Group learned and retained more with both V- and S-scores. These differences are consistent although not statistically reliable. In the lower section of Table VII are given for each group the percentages of *Ss* showing reminiscence and the mean number of lines of reminiscence obtained. Study of these data shows that there are no reliable differences between the Familiar Group and the Unfamiliar Group in either of these two measures of reminiscence.

Interpretation of these results is subject to the limitation of the accuracy of the method of grouping and the small number of *Ss* in the Familiar Group (14) who improved. The following conclusion, however, may be drawn from the present data: the familiarity or age of the learning-material has not been demonstrated to be a factor in the phenomenon of reminiscence. This stands in opposition to the conclusion of Ballard.

Inextricably woven into the problem of the effect of the age or familiarity of the learning material is the methodological question of the number of tests of retention. Improvement obtained in the recall of material, once incompletely learned, at several subsequent successive tests of retention after various intervals is not reminiscence, but is, instead, continued learning under conditions of distributed

TABLE VII

FAMILIARITY OF MATERIAL

				V-Scores					S-Scores				
		Age		Learning		Retention		% of R	Learning		Retention		% of R
Group	N	Mean	σM	Mean	σM	Mean	σM		Mean	σM	Mean	σM	
Fam.	44	9.90	.18	3.13	.40	3.15	.45	100.63	7.84	.63	8.31	.69	105.99
Unfam.	295	9.57	.07	2.19	.10	2.24	.12	102.28	6.35	.21	6.53	.22	102.83
D and σD		.33	.19	.94	.41	.91	.46	−1.65	1.49	.66	1.78	.72	3.16

	Ss showing reminiscence						Amount shown			
	V-Scores			S-Scores			V-Scores		S-Scores	
Group	N	%	σP	N	%	σP	Mean	σM	Mean	σM
Fam.	14	31.8	7.0	14	31.8	7.0	2.07	.31	2.64	.48
Unfam.	73	24.7	2.5	105	35.5	2.8	1.64	.11	2.32	.16
D and σD		7.1	7.4		−3.7	7.5	.43	.33	.32	.50

practice in which the practice-periods consist of recitations without knowledge of the accuracy of the performance. Examples of this fallacy are found in the experiments of Lobsien, Huguenin, the first, fifth and sixth experiments of Nicolai, and some of Ballard's experiments. It is obvious that the condition in which there are no formal tests of retention but in which *S* makes systematic attempts to review during the interval is an example of the same fallacy.

Effect of immediate recall. In fact, the experimental set-up of a formal learning-period followed by an immediate recall, and then, after any given interval, by a delayed recall is open to the above criticism, since the immediate recall is tantamount to a practice-period.

Brown called attention to this fact in the following statements; "in any second recall the learner has the benefit of practice or recitation in the first recall. Ballard seems to have ignored this factor."[45] The fact is, however, that Ballard

[45] Brown, *op. cit.*, 378.

seemed fully aware of the problem of the effect of the immediate recall and conducted an experiment to answer the question: "If the subjects had not been required to write out what they remembered immediately after learning, would the same amount of reminiscence have taken place, or was the effort made to reproduce the poem an essential factor in determining reminiscence?"[46] On the basis of two preliminary tests of memory an attempt was made to secure two equal groups, one of which had only a delayed recall after two days while the other had the benefit of an immediate recall also. Contradictory results were obtained which Ballard believed to be the outcome of several factors; viz., the small number (13) of *Ss* in each group; the manifest superiority of one group; and the unequal difficulty of the two selections of poetry learned. He concluded, however, "that the balance of advantage is on the side of the double test rather than the single test," and thus, "the act of writing out the lines immediately after memorizing has an effect, but only a slight one, upon the subsequent reminiscence."[47]

Ballard, furthermore, called attention to a fact that is here worth noting. Under conditions of complete presentation and a self-prompting technique the line between learning and retention can be drawn only arbitrarily to satisfy experimental needs. For practical purposes, it can be said that a given period of time is devoted to learning and that an immediately subsequent period is devoted to a test of the retention of what was memorized during the first period. During the so-called learning period, however, *S*, in the process of memorizing, frequently tests the retention of what he has learned by self-recitations without looking at the material. The immediate recall or first retention-test differs from the learning-period in at least three respects: (1) it is an additional practice period; (2) the practice is in the form of writing; and (3) there is no opportunity for self-prompting.

With the *Aussage* technique, the differences between the learning, or exposure-period, and the recall, or reproduction, is somewhat greater in that the exposure-time is usually so short that it limits opportunity for self-testing. The effect of the immediate recall upon reminiscence under these conditions was studied by Nicolai in his third experiment. Comparisons were made between the records of the same *Ss* with an immediate recall of one set of material and without an immediate recall of another set. The number of *Ss*, however, was very small (not over 8 to a group) and the equality of the two sets of material with respect to susceptibility to reminiscence was not taken into account. Furthermore, the data are given only in terms of the average score for each group. His results are of doubtful reliability, but it is worth noting that when the immediate recall was omitted no improvement was obtained with the 12- and 13-yr.-olds, while the *Ss* of 10 and 11 yr. showed improvement in only 2 out of the 5 intervals.

In an experiment on incidental learning, Myers' conclusion, based on results of indeterminable reliability, that an immediate recall has a beneficial effect upon the final recall throws no light upon the problem of reminiscence since his data are in terms of group-percentages of retention.

While it is highly probable that the immediate recall affects the amount of subsequent reminiscence the available data do not answer Ballard's question whether it is an essential factor in determining the phenomenon.

[46] Ballard, *op. cit.*, 36. [47] *Ibid.*, 37.

Method of learning. No previous work has been done on the effect of the method of learning upon reminiscence. In Experiment II, 161 *Ss* learned 24 lines of poetry by the whole-method and 149 by the part-method. The learning methods have been described in detail in a previous section of this paper. No reliable differences were found between the two methods in the three different measures of reminiscence; the percentages of retention of the group, the percentages of children exhibiting reminiscence, and the mean number of lines of improvement obtained, with the one exception, that with the S-scores reminiscence occurred in $35.4 \pm 3.8\%$ of the *Ss* who learned by the whole-method but only $20.8 \pm 3.3\%$ of those who learned by the part-method. The difference here of 14.6 has sigma of 5.0. A further analysis of the data revealed that this difference in the percentage of *Ss* showing reminiscence was limited to those children who reported reviewing: with the whole-method $39.1 \pm 4.7\%$ of the 110 *Ss* who yielded reminiscence compared to $20.0 \pm 3.8\%$ of the 110 reviewing *Ss* with the part-method. This difference of 19.1 has a sigma of 6.0. There were no reliable differences with the children who did not review. It thus appears that the method of learning has a differential effect with respect to reminiscence upon the *Ss* in whom rehearsal occurs. It should be remembered, however, (see Table II) that *within* each method of learning there were no reliable differences between the reviewing and the non-reviewing *Ss* in the percentages exhibiting reminiscence.

Our data suggest that the method of learning may be a factor affecting the phenomenon of reminiscence, although they do not justify any generalization regarding the relationship.

Age. Concerning the factor of age and reminiscence Ballard drew the following conclusions.

"The degree of improvement varies considerably with the age of the subjects. As a general rule, children of six years of age improve to the extent of 50 or 60 per cent in two days; children of twelve years improve from 10 to 20 per cent; subjects over 20 years of age do not improve at all." "If the number of subjects who at a given test show improvement is considered instead of the improvement in the aggregate score of the subjects, the same kind of difference is manifested. Nearly 90 per cent of infants improve, about 75 per cent of the older children, and about 30 per cent of students at a training college."[48]

The above conclusions refer to relative improvement, *i.e.* the ratio of the number of lines recovered to the number of lines learned initially. Ballard also drew the following conclusion, which has received much less attention:

"The absolute number of lines recovered, however, increases up to early adolescence and then gradually diminishes."[49]

It should be remembered, as Williams pointed out, that these conclusions of Ballard are not justified because of the small number of adult *Ss* studied, the lack

[48] Ballard, *op. cit.*, 79 f. [49] *Ibid.*, 80.

of uniformity through the different age-levels in the experimental conditions including the material learned, the method of presentation, the degree of learning, the method of measuring reminiscence, and also the amount of practice the *Ss* had had in such experiments. It is worth analyzing, however, the results obtained by Ballard with one group of children whose average age was about 6 yr. and two older groups whose average age was about 12 yr. The data are summarized in Table VIII. Reminiscence was measured in all groups by the percentage of *Ss* showing improvement, but the three groups differed in the material learned and the time allotted to learning, while the younger Group A varied from the two older Groups B and C in the method of presentation and in practice. The differences between Group A and Group B, Group A and Group C, and those between Group B and Group C and their sigmas, computed by the writer, are also given in Table VIII. Inspection of these data reveals that, with the exception of the 6-day interval, reminiscence was obtained with a reliably greater percentage of

TABLE VIII

AGE AND REMINISCENCE

(Percentage of Ss showing reminiscence)

			1 day		2 days		3 days		4 days		5 days		6 days	
Group	P*	T*	N	%	N	%	N	%	N	%	N	%	N	%
A	10	IV	288	75.0	282	86.5	274	88.7	246	88.6	236	75.0	250	55.6
B	8	II	287	58.2	681	75.0	174	58.0	595	50.4	118	57.6	56	44.6
C	4	I	644	45.5	841	55.3	646	47.5	1086	40.6	657	42.2	660	30.0

Differences in Percentages

	D	σD	D	σD	D	σD	D	σD	D	σD	D	σD
A—B	16.8	3.9	11.5	2.7	30.7	4.3	38.2	2.9	17.4	5.4	11.0	7.3
A—C	29.5	3.2	31.2	2.7	41.2	2.8	48.0	2.5	32.8	3.4	25.6	3.6
B—C	12.7	3.5	19.7	2.4	10.5	4.3	9.8	2.5	15.4	5.0	14.6	6.8

* Page and Table in Ballard's article.

younger children, Group A, than with older children, Group B, and that likewise all the differences between Group A and Group C which are in favor of the younger children are reliable. Also valid, however, in 4 out of the 6 intervals, are the differences between Group B and Group C, children of the same age level who learned different materials. It is, moreover, interesting to note that the size of the differences between Group B and C compares favorably with those between Group A and B. Since these data show that reminiscence varies as much with different poems learned by children of the same age as with children of different age levels who learned different poems, it is evident that Ballard was unjustified in attributing the variation in reminiscence to the age factor alone. Furthermore, the data of an experiment in which the same poem and procedure were used with children of different age-levels, ranging in average age from 8·7 to 12·6 yr., do not support his conclusion that reminiscence varies inversely with age.[50] Here the differences with both measures of reminiscence, the percentage of *Ss* showing reminiscence and the percentage of improvement, between the two 10-yr.-old groups and between the two 12-yr.-old groups are as large as, or larger than, the differences

[50] Ballard, *op. cit., Brit. J. Psychol., Monog. Suppl.*, 1, 1913, (no. 11), 13.

between the terminal groups of the age range studied, *i.e.* the 8·7-yr. and the 12·6-yr. groups.

Attention has already been called (section on Method of Measuring Reminiscence) to the inadequacy of the percentage of retention of the group as a measure of reminiscence and to the discrepancy in results between absolute and relative improvement. Ballard did not publish the data necessary for a statistical test of the reliability of the conclusion regarding the positive relationship between age and the absolute number of lines recovered, but the lack of uniformity in his experimental conditions would seriously limit the value of such a statistical analysis.

Williams used four age-groups whose average ages were 9.6, 12.7, 16.2 yr. and adult (age not given). With poetry, he obtained percentages of retention over 100 with the two younger groups but less than 100 with the two older groups. He concluded that reminiscence is a function of the age of the *Ss*. Attention is called, however, to the fact that Williams' data are in terms of the percentage of retention of the group and his published results, therefore, do not justify the conclusion that reminiscence varies with age.

TABLE IX

AGE AND REMINISCENCE

(Percentage of *Ss* showing reminiscence)

Age (in yr.)	Picture			Binet Object-Card		
	N	Nar. %	Inter. %	N	Nar. %	Inter. %
9	27	51	37	21	67	67
10	25	40	20	32	71	67
11	19	68	31	29	62	62
12	24	70	58	23	56	56
13	16	93	37	18	67	67

In the two experiments reported in this paper, the age-levels in which the number of *Ss* was sufficient to permit statistical treatment were as follows: Experiment I, ages 8, 9, and 10 yr. with 86, 120, and 40 *Ss* respectively; Experiment II, ages 9, 10, and 11 yr. with 73, 122 and 115 *Ss* respectively. In neither experiment were there any reliable or consistent differences between the ages studied in reminiscence measured in terms of the percentages of *Ss* exhibiting reminiscence or in the average amount shown.

The lack of dependence of reminiscence upon age is again seen in the data summarized in Table IX taken from McGeoch's study[51] in which reminiscence was obtained with the *Aussage* technique. Study of the table reveals that there is no consistent tendency for the percentage of *Ss* yielding reminiscence to vary with age in either form of recall, narrative or interrogatory, with either type of material, picture or object-card.

The conclusion that reminiscence is not a function of age is further substantiated by evidence from the experiments in which reminiscence was incidentally studied. The data from the Colvin and Myers study in terms of the percentage of correctness for each grade, from the third through the twelfth, and for college students show no consistent tendency for reminiscence to vary with age. Myers' re-

[51] McGeoch, *op. cit.*, M.A. thesis, 13, 16, 18, 20.

sults,[82] in terms of the percentage of *Ss* exhibiting reminiscence, with sixth, seventh, and eighth grade children and high school pupils show no reliable differences between the age-levels. The data published by King and Homan show that a greater percentage of the older *Ss*, 110 college students and 72 high school pupils, exhibit improvement than the younger *Ss*, 64 elementary school children. Although the difference between the college students and the elementary pupils is reliable, this result cannot be interpreted as demonstrating an inverse correlation between age and reminiscence since the learning materials, meaningful prose, varied with the different age-levels.

Analysis of the published data has not supported Ballard's conclusion that the relationship between age and reminiscence is negative in terms of relative reminiscence and positive in terms of absolute reminiscence. The relationship has not yet been established.

Time interval. There are two questions concerning the effect of the time interval upon reminiscence: (1) After what intervals does reminiscence occur? (2) Is there an optimal interval?

Data relevant to the first question are found in Nicolai's second experiment in which he obtained reminiscence with the *Aussage* technique after as short an interval as 30 min. and as long an interval as 5 days as well as after intervenient intervals. With the same technique McGeoch found reminiscence after 3 weeks. With poetry, Huguenin obtained it after 1 week, and Ballard after various intervals from 1 day to 2 weeks. The extremes of the intervals mentioned here are limited, of course, by the paucity of the experimental data. The literature contains nothing contrary to the supposition that reminiscence could be obtained with shorter or longer intervals than those studied.

Analysis of the data inevitably leads to the second question. It is obvious that in this connection the results from those experiments in which the same *Ss* recalled the same material after several successive intervals cannot be used (see section on Familiarity of Material). Ballard gave a very definite answer to this question in his following conclusion: "In the case of very young children the interval which secures maximal reminiscence seems to be three days. For older children it is undoubtedly two days. Individuals differ considerably, but as a general rule two days afford the most favorable opportunity."[83] Ballard's data do not justify the generality of this conclusion. It is true that in his two most extensive experiments with older children, between 11 and 14 yr. of age, the results show that a larger percentage of *Ss* exhibited reminiscence after an interval of 2 days than after intervals of 1, 3, 4, 5, 6, or 7 days. Computation of the sigmas of proportions for the percentages given yielded the following results: (1) with "The Ancient Mariner" the 2-day interval is reliably superior to all other intervals, but there are no valid differences between the other intervals up to 6 days, after which there is a significant decrease in the percentage of *Ss* who improved; (2) with "The Wreck of the Hesperus" the results are the same, except that the significant drop occurs at the 7-day interval between which and all other intervals there are re-

[82] Myers, *op. cit., J. Educ. Psychol.*, 8, 1917, 172.
[83] Ballard, *op. cit.*, 31, 80.

liable differences. In less extensive experiments with children of the same age who learned nonsense verses there is no reliable difference between the 2- and 3-day interval, but there is a significant tendency for these two intervals to be superior to the intervals of 1, 4, 5, 6, or 7 days. It is thus apparent that the optimal interval for older children is not "undoubtedly two days," but that it varies with experimental conditions. Furthermore, the data on very young children, from 5–7 yr., do not support the conclusion that the 3-day interval is optimal. Statistical analysis of the results of an extensive experiment on young children revealed that there are no valid differences between the 2-, 3-, or 4-day intervals (the percentages are respectively, 86.5, 88.7, 88.6) although these three intervals are significantly superior to the 1-, 5-, or 6-day intervals. It should be noted, moreover, in connection with this experiment that the same children were used for each interval but the material learned was not constant for all *Ss* at any given interval. Further evidence negating the generality of Ballard's conclusion is furnished by the results of an experiment with adult *Ss*. Here the percentages of *Ss* showing reminiscence is smallest for the 2-day interval, but the number of cases for this interval, as well as for the 5-day interval, is too small to permit statistical treatment. There are, however, no valid differences between the 1-, 3-, or 7-day intervals.

It is apparent that only under certain experimental conditions did Ballard find a reliably optimal interval or intervals. In the light of our analysis of his data his further conclusion that the predominance of motor elements, rhythm and alliteration, in the poetry tends to lengthen the interval can be considered only as an interesting suggestion. Before leaving the discussion of Ballard's study, it is worth noting that in those experiments in which he found a reliably optimal interval, this interval was not the shortest, *i.e.* the 1-day interval.

The number of *Ss* used by Williams at the five different intervals he studied makes his experiment significant, but his data throw little light on reminiscence since his results which are in terms of group-averages reveal nothing about the phenomenon for those intervals in which the group-percentage of retention indicates forgetting.

Statistical analysis of the experimental data on the effect of the time interval upon reminiscence has shown that the optimal interval varies with the experimental conditions among which are probably the material learned and the age of the *Ss*, besides other factors none of which has been isolated.

Other Factors. Among other factors upon which the phenomenon of reminiscence may depend are the practice-level in learning and recalling under conditions approximating those of the experiment, and the state of the *Ss* at the time of learning and of recall. Adequate experimental data on these factors is lacking, although Ballard[84] and Nicolai[85] did some work on the second factor in their studies of the effect of fatigue upon reminiscence. Their experiments were poorly controlled and their results lack reliability. It should be noted here, however, that fatigue as an explanation of reminiscence has been ruled out by Ballard's cogent objections and the evidence from the *Aussage* experiments in which the learning-time is a matter of seconds. Another factor that awaits research is the constancy of the phenomenon

[84] Ballard, *op. cit.*, 57-62. [85] Nicolai, *op. cit.*, 146.

with a given *S*, *i.e.* over and above all other factors, are there 'reminiscencers' and 'non-reminiscencers'?

SUMMARY

The results of the two experiments reported in this paper indicate that reminiscence, the improvement in memory after an interval of time without intervenient formal review or relearning, occurs independently of casual revival or intentional review. Reminiscence, consequently, merits attention as a valid phenomenon accompanying disuse. A critical analysis of the available experimental data permits the following conclusions.

(1) Reminiscence is probably a much more widely prevalent phenomenon than is revealed by many experiments on retention the results of which are in terms of the average scores of the whole group, which scores are inadequate and equivocal measures of reminiscence. The percentages of *S*s exhibiting reminiscence and the amount shown by them are adequate and valid measures of reminiscence.

(2) Whether the immediate recall is an essential factor in determining reminiscence has not yet been established.

(3) The factors of age, sex, intelligence, and familiarity of the learning material have not been demonstrated to affect reminiscence.

(4) The data do not permit formulations of the precise relationships between reminiscence and the nature of the material, the time interval, or the method of learning.

In the light of the meagerness of our present knowledge of the conditioning factors of reminiscence, an attempt at an explanation of the phenomenon is untimely. The experimental data published since Ballard's study throw little light on the relative merits of the two main hypotheses, inhibition and perserveration, which he discussed. It should be noted, however, that reminiscence is related to such phenomena as the superiority of distributed practice in learning, recovery of conditioned responses during an interval of inactivity following experimental extinction, and the work of Zeigarnik[56] on the retention of incompleted tasks. The bearing of reminiscence upon the law of disuse as an explanation of forgetting has been developed elsewhere.[57]

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[56] B. Zeigarnik, Das Behalten erledigter und unerledigter Handlungen, *Psychol. Forsch.*, 9, 1927, 1-85.

[57] J. A. McGeoch, Forgetting and the law of disuse, *Psychol. Rev.*, 39, 1932, 352-370.

GROWTH OF 'MEMORY IMAGES'

By WARNER BROWN, University of California

An experiment by Wulf[1] has been so widely quoted that it has seemed to me worth while to collect further data and to reëxamine some of the dicta in the light of earlier as well as of recent work.

Wulf's findings are marshalled against Müller[2] and particularly against the view that memory images tend to become unclear and to converge upon a common type much as the test-letters of an optometrist when viewed from too great a distance blur and become indistinguishable. Wulf finds, on the contrary, as he thinks, that the reproductions show two distinct tendencies (*Präzisierung* or featuring and *Nivellierung* or leveling) both of which result in the production of a more individualized figure preserving the basic traits of the original as determined by its structure. Instead of deteriorating, the images (or the later reproductions) show a positive phenomenon of growth.

Such findings were by no means new when Wulf's paper was published, but they had been differently, and I suspect better, explained.

Kuhlmann,[3] whose experiment anticipates Wulf's in many important respects, noted that some errors "would remain the same in character but increase in degree in the successive reproductions." He found three important sources of error, (a) ambiguous verbal description of the figure, (b) influence of associations with real objects which cause the reproductions to become more like the objects, (c) influence of "standard forms," *i.e.* regularizing in terms of symmetry, parallelism, conventional geometrical patterns.[4]

Piéron[5] described almost exactly the same phenomenon in the case of figures consisting of straight lines intersecting at various angles. He says:[6]

"So, even with these designs of irregular form, intellectualization, which generally supplements sensorial memory, intervenes often to a considerable extent. But the essential form of the memories involved in this experiment is motor, a succession or series of elementary kinesthetic images, generally with intellectual or visual support for the arrangement of the different isolated movements, and with dangerous

* Accepted for publication July 21, 1933.

[1] F. Wulf, Über die Veränderung von Vorstellungen (Gedächtnis und Gestalt). *Psychol. Forsch.*, 1, 1922. 333-373.

[2] G. E. Müller, III. Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes, *Zsch. f. Psychol., Ergd.*, 8, 1913, 1-567.

[3] F. Kuhlmann, On the analysis of the memory consciousness; A study in the mental imagery and memory of meaningless visual forms, *Psychol. Rev.*, 13, 1906, 316-348.

[4] *Ibid.*, 334.

[5] H. Piéron, Recherches comparatives sur la mémoire des formes et celle des chiffres. *Année psychol.*, 21, 1914-1919, 119-148.

[6] *Ibid.*, 145-146.

attempts at simplifying schematization which makes possible the substitution of symbols." And in a footnote to this: "Besides the attempts at schematization there is produced a spontaneous phenomenon of *assimilation* well known in human psychology, for example in the linguistic realm: two groups of analogous lines tend to be completely assimilated and identically reproduced and two lines slightly divergent are assimilated to a single direction and reproduced parallel."

He concludes:[7]

"But whatever the facts may be concerning the participation of the sensorial elements, it is none the less still true that memory is an associative phenomenon, a dynamic phenomenon of following a beaten path, always subject to the same set of fundamental laws."

Recently Perkins[8] using figures more meaningful than those of Piéron finds the same general and consistent growth in the direction of regularizing that was established by Kuhlmann and by Piéron.

Binet[9] had noted in the reports from children who looked at his card of objects, the tendency for the essential core of the thing perceived to be retained while non-essential detail fell away.

"The object seen on the card has an individuality which is seldom mistaken. In the case of the penny the effigy on it may be forgotten, or the worn contours, but not that it is a penny. No pupil has reported: 'I remember seeing at the top of the card a round object, obscure, I don't known now what it was.' The same can be said for the stamp; errors abound concerning its color, value, cancellation mark, etc., but no pupil has reported: 'I remember a small bright surface of such and such a color'."[10]

In the field of *Aussage* experiments such results have been the rule ever since the pioneer work of Binet and Stern.[11] In the year 1900 Binet wrote:[12]

"It is important to distinguish *logical errors* (in memory) or errors due to routine, such as imagining for example a thread or a pin in order to account for the attachment to the card of the button or tag, from *errors of invention* which consist in constructing an object which has no apparent connection with reality and which, consequently, one can not explain. The last kind is much less frequent than the former."

In 1905 he wrote:[13]

"It seems to me that the errors of testimony are not due entirely to memory; they are, to be sure, conditioned in a negative way by gaps in memory, but if the subject were aware of these gaps and took account of them he would not make mistakes. One makes mistakes because one employs reason and imagination unconsciously. There is reason to believe that the assumed troubles of memory depend on faculties quite other than memory, above all on judgment."

That memory reports grow, and may even improve after an interval of time was early well established, as can be seen for instance by consulting the references in

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[7] Piéron, *op. cit.*, 148.
[8] F. T. Perkins, Symmetry in visual recall, this JOURNAL, 44, 1932, 473-490.
[9] A. Binet, *La suggestibilité*, 1900, 282.
[10] *Ibid.*, 282 f.
[11] L. W. Stern, *Beiträge zur Psychologie der Aussage*, 1903-6.
[12] *Op. cit.*, 284.
[13] A. Binet, La science du témoinage, *Année psychol.*, 11, 1905, 128-136, esp. 134.

Whipple's handbook,[14] published some twelve years before the appearance of Wulf's paper. Whipple says:[15]

"The following narrative (report) . . . shows clearly the tendency of an adult *S* to describe a situation, a meaningful whole, rather than to enumerate details, as do many children. Indeed, the detail here is distinctly subordinated to the interpretative rendering. The narrative tells what the picture is about, rather than what it is."

Concerning the effect of repeating a report, he adds:

"It tends in part (1) to establish in mind the items reported, whether true or false, and (2) it tends to induce some departure in later reports, because these are based more upon the memory of the verbal statements of the earlier reports than upon the original experience itself, *i.e.*, the later reports undergo distortion on account of the flexibility of verbal expression."[16]

In 1915 Whipple gave an analytical summary of recall by Shultz including the following items:

"There takes place a process of logical elaboration, the effect of which is to emphasize the kernel of the episode, and to minimize unessential details (principle of conscious economy). This tendency also operates to distort reports so as to make them conform to what the witness regards as the natural course of events."[17]

Schultz says:

"It is apparent that the kernel of the episode is particularly emphasized in the recall and that unessential elements are left out; further, that a strictly logical continuity is built up where it was originally only suggested or even completely absent . . . the deciding reason for the appearance of the single elements is their *logical value.*"[18]

Recently Lewis used 100-word "legends" (narratives) and, in the absence of specific instructions to report the actual words, found that the recall tended to preserve the general contextual meaning.[19] This result is consistent with what has generally been found in *Aussage* experiments.

Ballard,[20] in 1913, showed that the amount that can be recalled after an interval may exceed in actual quantity the amount which could be recalled immediately after learning. A theory of neural growth or "positive brain change" was advanced as the least objectionable explanation for this anomalous finding. A subsequent study,[21] using lists of words confirmed the fact of growth to which Ballard had given the name reminiscence but indicated that at least some of the additions may be due to the inclusion of new items which, by chance, failed to appear on the first recall, while old items are retained because of the fixating effects of the first recall.

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[14] G. M. Whipple, *Manual of Mental and Physical Tests,* 1910, 311.
[15] *Ibid.,* 303-304.
[16] *Ibid.,* 309-310.
[17] G. M. Whipple, *Manual of Mental and Physical Tests,* 1915, 404.
[18] G. Schultz, Zur Aussagepsychologie: prinzipielle Erörterungen im Anschluss an ein zweifaches Experiment, *Zsch. f. ang. Psychol.,* 7, 1913, 547-574, esp. 561.
[19] F. H. Lewis, Note on the doctrine of memory traces, *Psychol. Rev.,* 40, 1933, 90-96.
[20] P. B. Ballard, Obliviscence and reminiscence, *Brit. J. Psychol., Mon. Sup.,* 1, 1913, (no. 2), 1-82.
[21] W. Brown, To what extent is memory measured by a single recall? *J. Exper. Psychol.,* 6, 1923, 377-382.

Thus without making an exhaustive search of the literature it seems clear to me that the fact had been well established before the time of Wulf's experiment, and since fully confirmed, that there is a growth of the memory image in the direction of greater individualization and more logical organization. Wulf's findings were not new; but his interpretation is new in that he ascribes the growth to the *Struktur* of the items themselves and repudiates all those principles of explanation upon which psychologists have been accustomed to lean: perseveration; Jost's law; assimilation; the forgetting of disconnected detail; the retention of significant (associated) parts; the integration of these significant parts into a relatively well rationalized whole "story," possessing in terms of the experience of the individual a maximum of credibility; substitution of parts or distortion of the whole in order to make possible such a coherent "story."

But Wulf was not satisfied with his successful (with respect to his particular material) attack on Müller's theory of convergence. He went on to attack, with the data of his experiment, the whole associationistic position. This attack rests upon the refusal to admit that a picture on paper can be *representative* in function. He says that he presented "objects, namely simple figures drawn on paper" (p. 335). But the type of structure which he calls *komprehensive,* and which he says was relatively common, was *not* a simple figure drawn on paper but was "a stair," "a lily," "a bridge," etc. Thus the drawing is endowed with the structural features of whatever object may be suggested by it. Instead of saying that a drawing represents, suggests or reminds one of an object, Wulf calmly asserts that it *is* the object and thus hurdles all the difficulties of the doctrine of association by one *tour de force.*

Katz had shown in 1913 that whether a figure is taken to be a picture of a thing or is taken to be lines on paper is a matter of individual type,[22] *i.e.* type of observer, not type of structure, as Wulf assumes. Kuhlmann had reported that "in every case the subject consciously sought for associations (for the visual figures) and the question, 'What is this form like?' or its equivalent, was probably always present in the subject's mind."[23]

About the time of Wulf's paper Granit suggested a genetic approach;[24] younger children more frequently reacted to the drawings as pictures of things. He assumed a mechanism of association by similarity.

The question of representation in such figures as Wulf's was attacked by Car-

[22] D. Katz, Über individuelle Verschiedenheiten bei der Auffassung von Figuren, *Zsch. f. Psychol.,* 65, 1913, 161-180.
[23] *Op. cit.,* 319.
[24] A. R. Granit, A study on the perception of form, *Brit. J. Psychol.,* 12, 1921, 223-247.

michael and his collaborators.[25] They found that they were able to predetermine to some extent apprehension of the figures by naming them. That is, the recall is found to be like the kind of thing named. This result is consistent with Wulf's claim that the recall is controlled by the structure as phenomenally apprehended, assuming that this structure is not the pattern of lines on the paper but is whatever the individual *sees in the picture.*

Gibson,[26] using figures which appear to be rather less meaningful than Wulf's, finds changes in recall which he interprets "as evidence for the existence of perceptual habits which have arisen in the individual during experience." In his recent book Bartlett,[27] after establishing very clearly the fact of growth in the memory image, presents very convincing evidence to show that the changes which occur and indeed the product of experience in the specific sense that they spring from recognizable influences in the social environment which govern the perceptual habits of the individual. If this contention is correct it greatly strengthens the environmentalist, empiricist, associationist interpretation of memory as opposed to the essentially nativistic interpretation of the Gestalt school.

New Experimental Data

For the present experiment 24 of Wulf's 26 figures were drawn to scale, enlarged, from the plate accompanying his article so that the longest were about 30 cm. long and the tallest about 25 cm. high. These were divided into 3 groups and each such set of 8 figures was shown to laboratory sections of an introductory course in psychology. His No. 1 and No. 5 were omitted in order to have eight for each of three groups; their design was not known at the time they were chosen for elimination. In Fig. 1 the designs are shown with Wulf's numbering. The figures in each of the three columns were presented one at a time in the order shown from top to bottom.

The exposure times (generally 10 sec., but for a few figures 5, 6, or 7 sec.) were Wulf's as was the 30-sec. interval during which the students were told to sit quietly but to avoid thinking about the design. At the end of 30 sec. the figure was drawn by the student and then answers were written to each of the following questions.

(1) On what data is the recall based?
(2) Was there a visual image?
(3) Does the reproduction seem to you to agree with the design shown?
(4) If you want to make any correction indicate what it should be, illustrating if necessary with a second drawing.
(5) Did you think of a name when the design was presented? What name?
(6) Does the design suggest any association?
(7) Does the design remind you of any experience or early impression?

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[25] L. Carmichael, H. P. Hogan, and A. A. Walter, An experimental study of the effect of language on the reproduction of visually perceived form, *J. Exper. Psychol.*, 15, 1932, 73-86.

[26] J. J. Gibson, The reproduction of visually perceived forms, *J. Exper. Psychol.*, 12, 1929, 1-39.

[27] F. C. Bartlett, *Remembering: A Study in Experimental and Social Psychology*, 1932, 1-317.

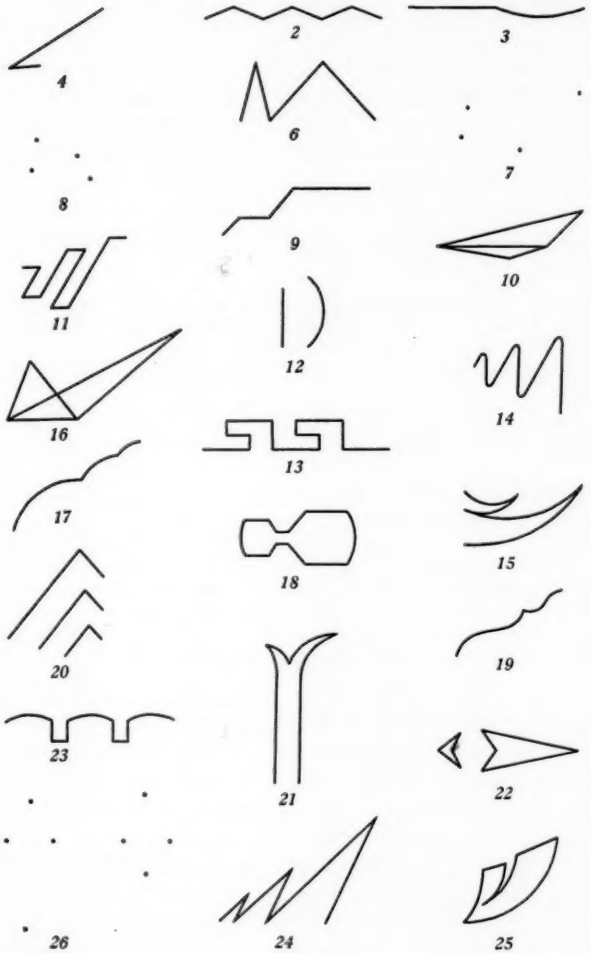

FIG. 1. DESIGNS USED IN EXPERIMENT
Wulf's Numbering

(8) Is there any specially striking feature about the design?
(9) Grade the accuracy of your drawing on a letter scale, *a, b, c, d.*
(10) What is the ground upon which you base your judgment of the accuracy?

In the present paper there is no discussion of these questions or the answers to them. Furthermore, no consideration is given to the original reproduction, which is treated as part of the learning period. The whole time devoted to the study of each figure was approximately 5 min., divided into four stages: (1) the period of observation, 5 to 10 sec.; (2) the 30-sec. period during which *S* was instructed "not to think about the figure;" (3) the making of a reproduction; (4) criticism of this reproduction and answering of the questionary about it.

There was no discussion of the experiment. On the other hand no intimation was given to the students that the experiment would be resumed. Then, after a period of from 14 to 17 days, without warning, a second reproduction was called for in conjunction with an examination. The instructions were as follows.

"Draw as well as you can each of the designs shown in the section-meeting in which designs were studied. Opposite each drawing state the features and/or associations which seem to be responsible for your remembering the design. Time: 15 min. for this test."

Eight spaces were provided on the page for drawing. No cues such as the part-figures used by Wulf were given.

A very few papers were discarded from students who could recall *no* figures; and a few from students who failed to give any data about features or associations. Then superfluous papers were discarded without inspection until, in the end, there remained three complete sets of 122 papers, 366 in all, each paper containing the attempt of one student to recall each of 8 figures, *i.e.* there were 122 attempts to recall each figure. The reproductions were scored by the writer.

Correctness. Each reproduction was judged first as to its correctness as a representative of the original. Although in the title of this paper I speak of the growth of memory images I am not, as a matter of fact, much interested in the memory image itself. I incline to think of remembering as delayed reaction. If, after a period of delay, the drawing is approximately what it would have been if *S* had copied it while he could still see it the test of delayed reaction has been successfully passed, the recall is "correct" and there has been *no* measurable change in the memory image. If, however, the test of delayed reaction is not a success, the reproduction is in "error." The expression "growth of the memory image" is merely a short way of saying that the delayed response is in certain respects different from what an immediate response would have been.

Table I shows the data for each group of 8 figures, and for each figure, with respect to *correctness* and with respect to *associations*. Comparisons

between the three groups give a rough measure of the extent to which the data might be affected by a different grouping of figures or by the use of a larger number of figures.

TABLE I

GROUPING OF WULF'S FIGURES; NUMBER CORRECT; NUMBER WRONG; AND WHETHER WITH OR WITHOUT ASSOCIATION

Figures	Right			Wrong			Total		All
	With	Without	Total	With	Without	Total	With	Without	
				GROUP 1					
3	46	26	72	24	9	33	70	35	105
7	9	6	15	51	30	81	60	36	96
10	17	4	21	45	14	59	62	18	80
14	12	2	14	50	27	77	62	29	91
15	31	8	39	21	19	40	52	27	79
19	12	6	18	30	6	36	42	12	54
22	34	9	43	13	12	25	47	21	78
25	2	3	5	19	28	47	21	31	52
Total	163	64	227	253	145	398	416	209	625
				GROUP 2					
2	34	13	47	25	14	39	59	27	86
6	12	6	18	45	17	62	57	23	80
9	8	5	13	70	14	84	78	19	97
12	12	4	16	38	17	55	50	21	71
13	46	10	56	31	13	44	77	23	100
18	19	1	20	64	22	86	83	23	106
21	20	8	28	30	6	36	50	14	64
24	36	7	43	35	14	49	71	21	92
Total	187	54	241	338	117	455	525	171	696
				GROUP 3					
4	16	7	23	65	27	92	81	34	115
8	23	17	40	22	12	34	45	29	74
11	17	14	31	39	25	64	56	39	95
16	55	7	62	16	4	20	71	11	82
17	42	11	53	24	0	24	66	11	77
20	14	5	19	18	6	24	32	11	43
23	34	12	46	49	6	55	83	18	101
26	31	17	48	30	14	44	61	31	92
Total	232	90	322	263	94	357	495	184	679
Grand Total	582	208	790	854	356	1210	1436	564	2000

It may be noted as a matter of incidental interest that the average student recalls in some recognizable way 5.46 of the 8 figures he studied a fortnight before and that 2.16 are approximately correct.

Associations. Two thousand figures were reported and 1436 of these were accompanied by verbal descriptions or associations. At first I divided

these associations into those in which the figure was taken to be a picture of a thing, "a bridge," "lightning," etc. (Wulf's *komprehensive* type) and those in which an analysis was given, "two triangles on one base," "eight dots," etc. (*isolative* type), but as this distinction did not prove useful these categories were combined into one, "associations."

Of the whole 2000 recalls 790, or 39.5%, are correct. Of 1436 *with* associations 40.5% are correct and of 564 *without* associations 36.9% are correct.

The very large number of items recalled (correctly as well as incorrectly) without associations is not due to failure to understand the directions as no paper was counted upon which there did not appear at least one item for which association was reported.

It seems clear that many of the delayed responses (approximately 28%) are effected without the occurrence of associations of sufficient vividness to compel the *S*s to write them down; and the responses made with no associations (or with much less vivid ones) are not, on that account, significantly less likely to be correct.

Errors. Errors were at first classified into 10 sorts under heads suggested by Wulf, Gibson, Perkins, and others, but as the distinctions between some of these classes proved, in practice, to be unworkable, the classes were combined until only 6 remained.

Table II gives the occurrence of each type of error for each figure subdivided under *association present* ('with') or *no association* ('without'). Table III (obtained by adding columns in Table II) gives the number of errors of various sorts combined for all 24 figures.

Regulated errors. The commonest error is "regulation;" a tendency to draw the figures more symmetrical, more uniform, better balanced, etc., than the original. Regulation accounts for 33% of the errors and 20% of all the recalls in those cases in which associations are reported. It accounts for 31% of the erors and 24% of all recalls *without* association.

From this it appears that the tendency toward regularization is not intimately dependent upon the presence or absence of associations, meanings or connections.

Irregular errors. In total errors the largest class after *regular* is *irregular*. These are the figures in which there is *less* symmetry, *less* balance, *less* regularity in serial progression or proportion than in the original figure. *Irregular* does not include items which could be classed as *typical* (even though very irregular) nor those classed as *reduced* (see below).

The figures which most frequently appear in *irregular* form are: No. 10, 18 cases; No. 14, 20; No. 24, 18; and No. 16, 19. Two of these (No. 14 and 24) are figures in which there is a regular progression of similar members in a series. Failure to reproduce this serial relationship seems to me hard to explain in terms of the inherent *Struktur* of the original figure. In the cases of the other two figures (No.

10 and 16, in both of which there are two triangles with a common side) there is already an asymmetrical pattern. Distortion of these figures in reproduction might, of course, be explained in terms of a development of the original pattern, and in that case they should be catalogued under *typical* or *accented* (see below).

TABLE II

FREQUENCY OF EACH TYPE OF ERROR, WITH ASSOCIATION AND WITHOUT ASSOCIATION (LATTER IN ITALICS)

Figures in order	Regulated		Irregular		Typical		Reduced		Added		Accented	
in groups	With	Without	With	Without	With	Without	With	Without	With	Without	With	Without
3	0	*1*	8	*2*	6	*0*	8	*2*	0	*1*	*2*	*3*
7	36	*20*	*2*	*4*	6	*0*	*1*	*1*	6	*5*	0	*0*
10	25	*4*	14	*4*	*2*	*0*	*2*	*6*	*1*	*0*	*1*	*0*
14	12	*10*	9	*11*	16	*0*	6	*4*	7	*2*	0	*0*
15	*2*	*4*	7	*8*	*5*	*1*	*2*	*4*	*5*	*0*	0	*2*
19	12	*2*	6	*1*	6	*0*	*1*	*1*	*1*	*1*	*4*	*1*
22	*4*	*6*	3	*0*	*1*	*0*	*5*	*4*	0	*0*	0	*2*
25	14	*9*	3	*12*	*1*	*0*	*1*	*6*	0	*0*	0	*1*
2	*1*	*1*	*4*	*3*	*5*	*0*	*9*	*10*	*4*	*0*	*2*	*0*
6	*25*	*8*	*4*	*2*	*5*	*0*	*8*	*7*	*1*	*0*	*2*	*0*
9	*23*	*6*	*6*	*2*	*12*	*1*	*1*	*1*	*28*	*4*	*0*	*0*
12	*15*	*10*	*8*	*6*	*12*	*0*	*0*	*1*	*3*	*0*	*0*	*0*
13	*1*	*0*	*3*	*4*	*2*	*0*	*4*	*4*	*20*	*5*	*1*	*0*
18	*1*	*2*	*7*	*3*	*31*	*2*	*12*	*10*	*0*	*0*	*13*	*5*
21	*0*	*0*	*1*	*0*	*14*	*0*	*6*	*1*	*0*	*0*	*9*	*5*
24	*8*	*4*	*10*	*8*	*9*	*1*	*6*	*1*	*2*	*0*	*0*	*0*
4	43	23	3	4	19	0	0	0	0	0	0	0
8	12	8	1	2	6	0	2	2	0	0	1	0
11	13	13	5	9	8	0	1	1	11	2	1	0
16	1	0	15	4	0	0	0	0	0	0	0	0
17	12	0	2	0	1	0	5	0	3	0	1	0
20	4	3	0	2	9	0	3	1	0	0	2	0
23	1	0	3	1	3	0	17	2	24	3	1	0
26	17	5	5	7	7	0	0	2	1	0	0	0
Total	282	139	129	99	186	5	100	71	117	23	40	19

TABLE III

FREQUENCY OF RESPONSES
(Total 2000)

	With Association	Without Association	Total
Regulated error	282	139	421
Irregular, distorted, error	129	99	228
Typical error	186	5	191
Faded, reduced, part, error	100	71	171
Added error	117	23	140
Accented error	40	19	59
Total errors	854	356	1210
Total correct	582	208	790
Total recalled	1436	564	2000

They appeared to me to be merely less well formed, therefore more irregular, than the original.

Reduced. When the reproduction showed a lack of distinctness, fading, blurring, or when only a part of the original appeared, it was classed as "reduced." If this is what Müller[28] meant by *Undeutlichkeit,* then Wulf is right in saying that this type of error does not represent the characteristic course of change in the memory image. Such degenerative changes occur in only 14% of the errors and in only 8.6% of the whole number of recalls. It should be noted, however, that Müller expressly limits the principle of convergence to cases in which *individuals of the same class* tend to become indistinguishable (such as letters of the alphabet). He says[29] that if there is only one thing of a kind (such as a chess-piece) or if the thing can be separately named (presumably by a class-name) it will *not* tend to become like something else no matter how unclear the image. The figures used in this experiment are probably so distinct from one another in kind that the convergence principle does not apply.

There is, moreover, a striking phenomenon which should be observed in this connection although I am unable to reduce it to quantitative terms. The reproductions made 14 to 17 days after learning are quite uniformly *drawn with light, irregular and simple strokes,* whereas those made during the learning period were quite firmly drawn with stressed or repeated strokes so as to reproduce the appearance of *heavy lines* as in the originals. Almost all the later reproductions are in this sense *faded* or ghost-like.

The figures which most frequently appear in reduced form are: No. 2, 18 cases; No. 6, 15; No. 18, 22; and No. 23, 19.

A cross classification of *reduced* recalls according to the presence or absence of associations does not yield significant results.

Typical. It should be noted that no error was classed as *typical* if it could be called *regular. Typical* or normalized errors are those (irregular reproductions) in which the figure drawn is clearly representative of some object which the original figure might also represent (a kite, a bridge, a dog, a constellation of stars, etc.). Errors of this kind are, of course, confined almost entirely to the association category, for very rarely is a drawing so unmistakably a picture that one dares classify it in *typical* without the confirmation of a verbal statement.

Here, as in *Aussage* experiments, recall is determined by meaning or total significance, possibly retained in a purely verbal form, not in terms of the visual pattern of the original. Careful checking with the reproductions made immediately after study proves that the figures were not originally apprehended in these *typical* forms; at that time nearly all the drawings were fairly exact reproductions of the *actual figure* (the lines on the paper).

After two weeks nearly one quarter (22%) of all errors with associations are of the typical class.

Accented. I was able to find a relatively small number (5%) of errors which I could put in Wulf's class "accented" *(Pointierung).* In these cases some one aspect of the original figure dominates the recall or is found over-emphasized in the reproduction. Wulf's conditions were more favorable than mine for getting re-

[28] *Op. cit.,* 505-567. [29] *Ibid.,* 563.

liable measurements of small amounts of this "accenting,"[80] and it must be that I have overlooked many cases which he would count. There is an obvious difficulty in determining what aspect of a figure is to be considered characteristic. What appears to be a violent distortion of the original may in fact be an accenting of some feature which was, for the subject of the experiment, a salient characteristic. Items which I consider merely *irregular,* might on closer inspection or under the pressure of interest in a theory, be classified under *Pointierung.* I endeavored to trace some of the distorted figures back to the original act of apprehension through the reproductions made at the time of learning, but only in a few cases did the first drawings show significant distortions.

Added. A type of error which is relatively common (14% of those with associations and 6.5% of those without association) is that in which there is an increase in the number of parts of the figure while the essential structure of the figure itself is retained. Three of the 24 figures are responsible for 60% of the errors of this type. Figure 9 is assimilated to a series of plateaus. In 32 cases an extra step is added to this figure and in only 4 of these cases did S fail to report a meaningful association. Figure 23 is an incomplete series of arches often called a bridge. In 25 cases extra elements (piers or arches) were added to the reproduction of this figure and in only 5 of these cases did *S* fail to report an explanatory association. Figure 13 is frequently described as a Greek decorative design or as dogs' heads. In 27 cases added units were given in reproduction. This figure is not (like figure 23) incomplete from the point of view of internal structure, but it seems to be dissatisfying with respect to the number of units; and in that sense it may be regarded as incomplete structurally, but only 3 of the 27 instances were without verbal associations.

In the three figures in which *addition* is conspicuous, I am convinced that there is some connection between the making of additions and the occurrence of interpretative associations. I see no reason for believing that the *S*s analyzed these particular designs into discrete parts, counted the parts and later forgot the numbers. Of the 3 figures which do consist of separate parts (dots) No. 8 never shows an increase or decrease in the number of dots, No. 26 shows one increase, and No. 7 shows only a small number of increases or decreases. Additions occur either because they serve better to *represent* the object remembered, or else because of the operation of some feature of the structure of the thing represented. In either case the change (growth) which takes place in the memory image is not a change in the "trace" of the actual visual experience of a certain linear design but is a change connected with the thing (or idea of the thing) *represented by* the linear design.

From my point of view a change caused by the nature of the form itself ought to arise from what was *actually seen* and could be reproduced immediately afterward, *i.e.* a pattern of black lines on a white card. But the present results seem to show that, in certain cases at least, changes (growth additions) are made in the memory image which occur only when the figure (apart from its own features as a figure) suggests *something not actually seen.*

[80] His reproductions were comparable in size with the originals and could be checked by translucency, whereas my originals had to be large enough for exhibition to a group of observers.

SUMMARY

(1) After two weeks the recall of visual figures is accompanied in almost two-thirds of the instances by verbal, descriptive associations. But the presence or absence of these associations does not seem to have any material bearing upon the correctness of the recall.

(2) "Reduced" or "faded" reproductions (Müller's) are not conspicuously frequent. But neither are "accented" (Wulf's) examples.

(3) A considerable number of errors classed as "typical" (but not "regular") and "added" reveal the operation of associative processes of a logical sort dominated by the significance of the drawing as a representation of an object and clearly *not* understandable in terms of the linear figures actually presented. In these cases the reproduction may be *better than* the original as a representation of something of which the original was also a representation, but they are *different from* the original as perceptual forms.

(4) More than half the errors fall in the classes "regulated" or "irregular and distorted." Regulation may be due to associative correction or it may be due to the inherent qualities of the figure as such. Distortion and irregularity may be due to fading or may be due to accenting of what appears to the observer (but not to the experimenter) as a characteristic feature of the figure.

(5) In general the reproductions of these twenty-four figures, made by a large, heterogeneous population of students, can not be explained in one single way. Some of them undoubtedly show the influence of the linear characteristics of the seen figure; more of them do violence to the configuration as such and reflect the influence of meanings associated with the original figures; and a still larger number can be interpreted in either way.

THE AFFECTIVE CHARACTER OF THE MAJOR AND MINOR MODES IN MUSIC

By KATE HEVNER, University of Minnesota

This experiment is an investigation of the affective quality of the major and minor modes in music. It is an attempt to determine (1) whether the historically affirmed characteristics of the two modes are apparent to listeners, and (2) if they are, to what extent the recognition of these characteristics depends on training in music, on intelligence, or on the talent measured by the Seashore tests.

In general, it has been assumed by professional musicians and by interpreters of musical meaning that there is a difference in the affective qualities of the major and minor modes. The major is associated with the following characteristics: it is dynamic, an upward driving force; it is determining and defining, and more natural and fundamental than the minor; it expresses varying degrees of joy and excitement; it sounds bright, clear, sweet, hopeful, strong, and happy.

For the minor, the characteristic qualities include the following: it is passive, a downward drawing weight; it is determined and defined; it expresses gloom, despair, sorrow, grief, mystery, longing, obscurity, restlessness, melancholy; it is mournful, dark, depressing, doleful, dull, plaintive, and soothing.

The explanations given for these effects vary with the theory of consonance which is accepted.[1] They are attributed variously to combination and difference tones (Helmholtz), or to tonal fusion (Stumpf), or to conformity and noncomformity to a mental standard (Britan).[2] By those experimenters who do not accept the effects as inherent in the physical sounds of the two modes, they are explained in terms of specific training, individual differences, and previous experience (Downey,[3] Heinlein).[4] The necessity of demonstrating the qualities characteristic of the major and minor modes seems, to most professional musicians, ridiculous. They habitually use the minor mode to achieve certain effects, although they are of course very well aware that there are other devices by means of which these same effects may be produced. Variations in tempo, rhythm, harmony, intensity, and in the melodic

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[1] For a good brief review of the theories and explanations see C. P. Heinlein, The affective characters of the major and minor modes in music, *J. Comp. Psychol.*, 8, 1928, 101 f., and for a short review of the theories of consonance see R. C. Larson, Studies on Seashore's Measures of Musical Talent, *Psychol. Monog.*, 40, 1930, (no. 181), 33-73.

[2] H. H. Britan, *The Philosophy of Music*, 1911, 1-311.

[3] J. E. Downey, A musical experiment, this JOURNAL, 9, 1897, 63-69.

[4] *Op. cit.*

line are equally useful or perhaps more useful in carrying the meaning or expressiveness of the composition. Musicians also recognize that in producing its effect in the listener, the mode (major or minor) is never the sole factor; it is only a part of the total effect, inseparable from the other elements, rhythm, harmony, melody, intensity, and tempo, and is moreover always apprehended not in an isolated moment of time but as part of a sequence, influenced by what has been heard immediately previously. For example, the soothing effect of a certain composition may be due to any one of a number of different factors or to a combination of them; to the slow tempo, or low intensity, or smooth and simple harmony, or the even, unaccented rhythm, or to the contrast with a preceding animated and excited section. It would be difficult to extricate any one of these elements from the whole texture of the composition, and to study its effect in isolation, and this difficulty has made it impossible for experimenters to achieve stable and consistent and verifiable results.

Naturally the effect on the listeners of a sprightly minuet is cheerful and stirring, even though it is written for the most part in the minor mode, and the effect of a funeral march, with ponderous chords in slow tempo, is heavy and depressing even though it is written in the major key. Psychologists who have experimented with mood effects by presenting to their observers a number of compositions, of various kinds, sometimes in one mode, sometimes in another, have therefore always found great inconsistencies in the results, and they have been led to the conclusion that the effects are probably most largely determined not by the music itself but by something entirely extraneous, by the nature and complexity of the organic state of the listener, or by his specific training in interpreting music.

Experiments in which an attempt has been made to isolate the one factor, modality, from all the other elements, have also led to a denial of the historically affirmed characteristics of the two modes. The most recent and extensive experiment on the affective character of the major and minor is that of Heinlein.[5] This experimenter used 30 *O*s, whose training or lack of training in music had been previously ascertained. They listened and reported the effect of a number of different series of major and of minor chords, presented under very carefully controlled conditions. Each chord was presented alone, and reacted to as a separate unit. The intensity, the position, and the pitch were varied systematically, and the experimenter was able to show that intensity changes affect the complexion of a harmonic configuration. He states that "a change in intensity is capable of almost completely masking the historically affirmed effect provided by either mode."[6] Loud chords are rarely soothing either in major or minor, and soft chords are frequently soothing. Likewise "changes in pitch modify feeling tone reaction to chords of either mode. Chords, either major or minor, in the upper pitch register call forth a larger percentage of major responses than do chords in the lower pitch register. Reciprocally, chords of either mode in the lower register call forth a greater percentage of minor responses than do chords of either mode in the upper pitch register."[7] Heinlein is also of the opinion that, "the assumption long entertained by theorists in regard to the supposed intrinsic characters of the modes must be dismissed. It has been shown that reaction to harmonic configuration is variable for both trained and untrained *O*s. The mode in which a composition is written has little relation to the type of feeling which the composition may arouse. . . .

[5] *Op. cit.* [6] *Op. cit.*, 138. [7] *Op. cit.*, 140.

Any fixity of feeling tone in relation to a given mode is dependent upon training to react in a specific manner to a purely intellectual discrimination."[8]

But these conclusions are based on the study of chords only—chords in both modes, grouped and heard in fairly rapid succession, and there are a number of serious objections to the use of such material as stimuli for an experiment in expressiveness or mood effect. (1) In the first place the stimuli cannot be regarded as "music." The composers who acclaim the minor mode as dull, melancholy, plaintive, etc., have reference of course to its effect when it is used in musical compositions. (2) The expressiveness, especially if it is in the nature of a mood effect, requires more than one moment of time to be apprehended by the listener. The plaintiveness may perhaps lie only in the *accumulations* of certain sensations or perceptions, requiring a relatively long time for their apprehension. The rapid shifting of attitude required in listening to series of unrelated major and minor chords would certainly tend to obliterate any intrinsic differences which may be found in either. (3) Certain of the effects claimed for the different modes lie in the melodic skips, that is, in the interrelation of the tones which make up the two scales. Not all of the effects attributed to the minor are due to the harmony, for some of them are aroused by the peculiar qualities of the melodies written in the minor scales. These effects are totally absent when lone chords are used as stimuli.

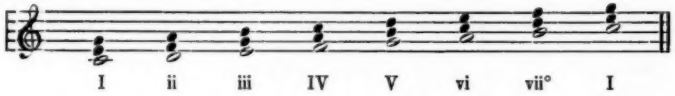
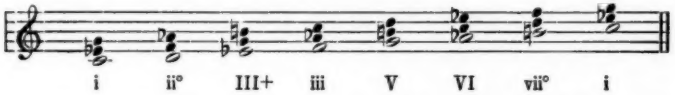
In order to obviate the difficulties in both the methods previously used for the study of the qualitative effects of these two modes, the method of presenting entirely different musical compositions, and the method of using single major or minor chords, the experimenter devised a new method of procedure. The stimuli employed were 10 short musical compositions, with two versions of each, one written in the major mode, the other in the minor mode. By this means, 10 pairs of stimuli were presented with every factor held constant except the modality. In each pair of compositions, the rhythm, tempo, and intensity were identical, and the only differences in melody or harmony were those occasioned by the transposition from one mode to the other. The difference, if any, in the effect on the listener, must therefore be attributed to modality, since every other factor in the two versions remained unchanged.

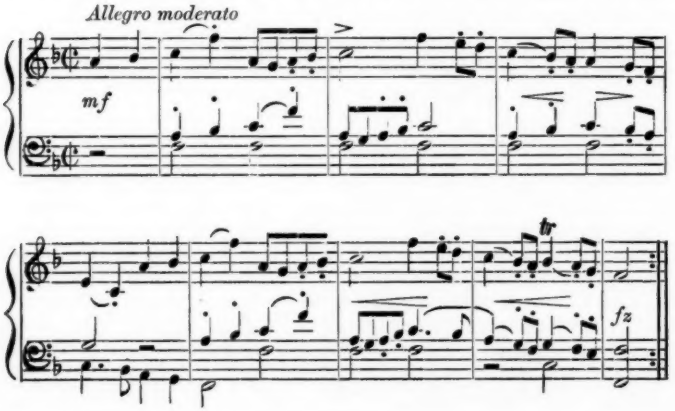
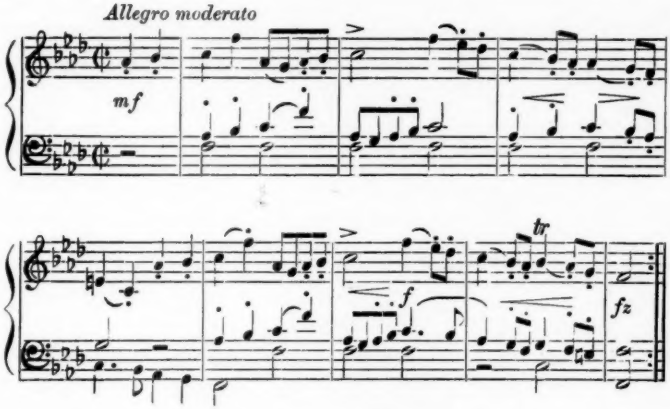
The compositions used for the experiment were all relatively short but complete musical ideas, and they were quite different in character, including gavottes, folk songs, minuets, rigaudons, andantes, themes from sonatas, etc., by Bach, Beethoven, Schumann, Rameau, Gluck, and Arensky. Some of them were written originally in the major and transposed to minor, and some appeared in the minor and were changed to major.

In considering the nature of the changes made, it is necessary to keep in mind the fundamental differences between the major and minor modes. There is one form of the major scale, and two forms of the minor, (a) the harmonic, with an augmented second between the 6th and 7th degrees, and, (b) the melodic, in

[8] *Op. cit.*, 140.

which the 6th and 7th tones are raised in the ascending form and lowered in the descending. We have employed the harmonic form for our music, since it is much more commonly used today.

Harmonic progressions are determined through the inherent tendencies of the triads built upon the tones of the scale to move to other triads of the scale. Triads which appear upon the tones of the major scale are listed below and an examination of them will show that there are three major, three minor, and one diminished triad.

The triad pattern of the minor scale appears as below and it is to be noted that there are two major, two minor, two diminished, and one augmented triad.

A comparison of these two series shows that V and vii° are identical in both major and minor, and that I, IV and vi in the major change to i, iv, and VI in the minor. We find no change in the dominant seventh and leading-tone triads, but a great change in the tonic, subdominant and submediant triads, the character of which shall determine the key feeling of our music. All the changes in the music for this experiment have been made with a view toward maintaining the triad arrangements as they appear in the two scales.

As an example of the two different versions which were contrasted in the experiment, we quote here from a little Gavotte by Martini. The original was written

in the major (key of F major), and the harmony alternates from tonic to dominant seventh. Since the V_7 chord in the major and minor are identical, this leaves only the tonic triad to be changed, but it is the tonic triad with its altered third, which gives the defining touch to the tonality.

The other items and their changes are as follows:

Schumann, *Folk Song from the Album for the Young* (Key of D Major).

Two changed versions are used in this example in order that each may be wholly major or minor. The selection in the original is in ternary form, A B A. A appears each time in minor, B in major. In the changed minor version B appears in minor and A is left unchanged, while the opposite occurs in the changed major version. There have been no changes in the scale degree of tones as they appear in the context, but changes in pitch are necessitated by the removal or insertion of accidentals.

Rameau, *Le Tambourin* (Key of E Minor), first part.

As in the Gavotte, harmonic motion swings to and fro between tonic and dominant. Thus the minor version depends largely on the tonic chord for its characterization, although the ornamental changing tones which are chromatically altered in the major version emphasize the key sense. A slight pitch change occurs in the transition after the second appearance of the main theme. In the original the creeping chromatic descent emphasizes the minor tendencies, and to check this emphasis, we omit the half-steps, G sharp, to G natural, to F sharp, by repeating the G sharp.

Arensky, *Chanson Russe,* from Transcriptions for the Young, edited by A. Siloti (Key of E Minor).

The element which gives this example its characteristic minor touch is the use of the minor scale triad arrangement, which is wholly cancelled in the major version, where many originally minor chords are changed to major.

Bach, *Musette,* from English Suite in G, edited by A. Wier (Key of G Major).

The original signature of one sharp has been changed to one of two flats. The most noticeable effect of this change is the constantly recurring B flat of the tonic

chord, which adds much to the minor feeling. In the seventh measure of the second section the descending melodic scale has been used.

Beethoven, *Minuet,* Op. 49, No. 2 (Key of G Major).

Minor feeling in the changed version depends mostly on the tonic chord here, because it is used in conjunction with the V_7, excluding for the most part all other chords. In the changed version there is a slight difference in the transition (measures 9–12). In the original this transition appears in the relative minor key of E, which, because of its close association with the major merely emphasizes the former tonality, upon its second appearance. In the minor, the transition is left in the original key, the only means of maintaining the original minor feeling without departing too much from the original pitch.

Durand, *Chaconne,* edited by A. Wier (Key of A Minor).

This example is dependent largely on the tonic and subdominant minor for its key feeling. Both of these chords are changed to major in the different version.

Gluck, *Andante (Orpheo)* (Key of F Major).

The changed version of the Andante achieves its minor feeling from the use of the triads in their minor scale arrangement. In addition to this the descending melodic minor scale is used in measure three of the second section. In measures nine and ten of the same section, it is felt that the harmonic form of the minor scale is more appropriate with the underlying harmony.

Rameau, *Rigaudon* (Key of E Major).

By adhering to the minor triad arrangement, and, as in Le Tambourin, changing notes which involve a tone of the minor instead of the major scale, the minor feeling is stressed in the changed version.

Bach, *Minuet,* edited by E. Hughes (Key of G Major).

A very simple harmonic scheme which involves the use of I, IV, V governs the first section. In the minor version I and IV become minor chords. In the second section the relative minor key is used leading us back into the original key. In the changed version, substitution of the subdominant key of the relative major has been used here, and though this is not a closely related key, it can be used without changing the staff degrees. By using an F sharp in the 12th measure of the second section, we anticipate the return to G. Minor.

These various compositions were presented to 4 different class groups, including in all 205 students, in the Elementary Laboratory (Sophomore) courses at the University of Minnesota. They were played by the same pianist, who had taken especial pains to play both versions of the same composition in an identical manner, and who, in the opinion of the *E,* was successful in so doing. Each group heard all of these compositions, 5 major versions and 5 minor versions. No student heard both the major and the minor version of any one composition. *E* gave names to the items, such as "Part of a Sonata in 3/4 time," "Song," "Study in 4/4 time," etc. During and immediately after the playing of each item, the *O*s checked from a mimeographed word list all the adjectives which seemed to them descriptive of the music. One list of adjectives was arranged in groups of opposites for the *O*s convenience, and another list was arranged with the words of similar connotation listed together. *O* was told to check as few or as many as he pleased, and a new copy of the list was furnished for each composition. The lists are as follows:

LIST I

Check all the terms in the following list which seem appropriate to the music. Check as many or as few as you like:

vigorous	tranquil	heavy	delicate
sad	happy	depressing	exalting
serious	light	exciting	soothing
longing	satisfying	obstinate	yielding
sacred	secular	frustrated	triumphant
restless	quiet	lyrical	dramatic
bright	dark	gloomy	gay
melancholy	merry		
robust	serene	sentimental	tragic
emphatic	leisurely	tender	plaintive
martial	hushed	passionate	mournful
dignified	dreamy	yearning	pathetic
majestic	vague	pleading	doleful
ponderous			
sober			
dramatic	weird	cheerful	clownish
impetuous	mysterious	exhilarated	humorous
agitated	awe-inspiring	joyous	whimsical
sensational	grotesque	soaring	farcical
stormy	gruesome	triumphant	playful
	fanatic		boorish
spiritual	horrible	graceful	
lofty	fantastic	fanciful	
solemn		quaint	
mystical		sprightly	

After the first playing of the composition, following a brief rest period, the same 10 versions were played again, and this time the *Os* were asked to check the one adjective in each pair of a list of opposites which best described the music.

LIST II

In the following pairs of terms, be sure to check one in each pair.

vigorous	tranquil	heavy	delicate
sad	happy	depressing	exalting
serious	light	exciting	soothing
longing	satisfying	obstinate	yielding
sacred	secular	frustrated	triumphant
restless	quiet	lyrical	dramatic
bright	dark	gloomy	gay
merry	melancholy	dull	spirited

What quality in the music is most important in giving it its character?

rhythm	tempo	harmony	melody

How does it appeal to you?

like	indifferent	dislike

Does the piece seem familiar? Do you think you have heard it before?

yes	no

Can you give the title or composer? _____

Results

(1) Tables I and II show the actual number of votes for the various adjectives tabulated from the first schedule blank, on which the student could check as many or as few adjectives as he chose. In Table I are all the adjectives which were checked more often for the major version than for the minor. These adjectives represent the characteristics of the major mode as they were perceived by this group of listeners. Likewise the list in Table II represents the adjectives which were associated with the minor. Besides showing the actual number of votes for each word, the tables rank the adjectives in order according to their importance in characterizing either

TABLE I

Showing Number of Votes for Major Adjectives for Major and Minor Versions

Adjective	Votes		D	Adjective	Votes		D
	Major	Minor	P.E.$_{diff.}$		Major	Minor	P.E.$_{diff.}$
Happy	316	102	17.27	Soaring	40	22	3.16*
Light	374	171	15.43	Leisurely	98	70	3.11*
Sprightly	251	81	14.95	Serene	91	68	2.71*
Cheerful	286	115	14.52	Whimsical	85	65	2.50*
Joyous	196	53	13.94	Delicate	134	109	2.42*
Gay	297	127	13.28	Robust	31	18	2.31*
Bright	260	108	12.96	Lyrical	146	121	2.31*
Merry	224	89	12.45	Vigorous	75	61	1.75*
Playful	203	75	10.85	Spiritual	34	27	1.23*
Graceful	243	144	8.15	Exalting	47	39	1.21*
Exhilarated	114	54	7.20	Clownish	7	3	1.08*
Satisfying	86	48	4.87	Farcical	8	4	1.08*
Quaint	93	61	3.88*	Triumphant	70	62	1.07*
Impetuous	45	25	3.85*	Soothing	97	90	0.78*
Fanciful	117	81	3.68*	Exciting	70	64	0.63*
Humorous	23	8	3.33*	Secular	23	22	0.22*
Sacred	36	17	3.16*				

* Ratios not statistically reliable.

major or minor. This rank order for the adjectives takes into account not only the size of the ratio of major to minor votes, but also the total number of persons voting for the various adjectives. For example, in Table I there were 374 votes for 'light' when a major version was played, and 171 votes when a minor version was heard, which gives a 2.18 to 1 majority for lightness as a characteristic of the major mode. Again, in Table II, there was 1 vote for 'stormy' when a major version was played, and 11 votes when a minor version was heard, which gives an 11 to 1 majority—an overwhelmingly large majority for stormy as a characteristic of the minor mode. But these ratios, 2.18 to 1 and 11 to 1, do not take into account the number of people voting, which is an important consideration, for in the case of 'light' there were 545 votes cast, and in the case of 'stormy' only 12. Since there were 205 *O*s, and each one listened to 5 major versions, the

highest possible number of votes for any one adjective was 1025, of which 545 is 53% and 12 is only 1.2%. Obviously some method for rating the importance of the adjectives in terms of the percentage of people voting for them was necessary, a scheme which would reduce the high majority ratios of such words as 'stormy' when the percentage of people voting was at a minimum. This was done by calculating the difference between the percentages of major and minor votes for each adjective, and the P.E. for each difference. The ratio of the difference to its P.E. ($D/P.E._{diff.}$) is a direct

TABLE II

SHOWING NUMBER OF VOTES FOR MINOR ADJECTIVES FOR MAJOR AND MINOR VERSIONS

Adjective	Votes		D	Adjective	Votes		D
	Major	Minor	$P.E._{diff.}$		Major	Minor	$P.E._{diff.}$
Pathetic	15	92	15.96	Hushed	27	49	3.55*
Melancholy	36	138	11.49	Ponderous	16	33	3.08*
Plaintive	47	142	10.69	Sentimental	47	71	3.03*
Yearning	39	113	8.78	Fantastic	8	20	2.86*
Mournful	28	92	8.08	Quiet	89	118	2.83*
Sad	50	121	7.93	Agitated	20	36	2.81*
Sober	49	110	7.24	Heavy	15	30	2.69*
Pleading	25	77	7.08	Tender	72	93	2.35*
Mysterious	6	47	7.02	Stormy	1	11	2.19*
Longing	68	133	6.74	Majestic	21	32	2.08*
Doleful	17	66	6.71	Dignified	69	82	1.53*
Gloomy	30	76	6.25	Grostesque	1	6	1.50*
Restless	85	149	6.20	Emphatic	38	45	1.21*
Weird	5	38	5.61	Passionate	12	18	1.19*
Dramatic	62	109	5.41	Solemn	89	100	1.16*
Mystical	9	35	5.21	Lofty	25	31	1.15*
Serious	86	136	5.16	Martial	10	14	1.08*
Depressing	36	76	5.13	Yielding	11	13	0.81*
Tragic	7	33	4.81	Tranquil	79	85	0.71*
Dreamy	66	105	4.47	Obstinate	3	5	0.54*
Frustrated	9	30	4.17	Awe-inspiring	1	8	0.27*
Vague	27	52	4.03	Sensational	5	6	0.27*
Dark	9	29	3.96*				

* Ratios not statistically reliable.

measure of the importance of the adjectives, and in Tables I and II the adjectives have been listed in order according to the sizes of these ratios.[8]

In both of the tables all the differences which are not statistically significant are marked with an asterisk (*), although in many of these cases the ratio of D to its P.E. is so close to 4 that it may be considered a satisfactory difference.

A glance through the list of adjectives in these tables, with the substantial majorities for many of them, shows a most striking adherence to the historically affirmed characteristics. For the minor mode the melancholy-

[8] The P.E. of the proportions and of the differences have been calculated from Yule's formulae: $P.E._p = .6745\sigma_p = (pq/n)^{\frac{1}{2}}$; $P.E._{diff.} = (P.E._{p_1}^2 + P.E._{p_2}^2)^{\frac{1}{2}}$.

mournful-gloomy-depressing quality probably stands out as of greatest importance, and then the plaintive-yearning-longing quality. It has also quite clearly a mysterious-mystical-weird-restless-dramatic effect, and a vague-dark-dreamy-hushed-sentimental aspect. The more extreme ideas, majestic, grotesque, sensational, etc., show negligible differentiation. For the major mode the greatest importance is given to such adjectives as happy, sprightly, cheerful, joyous, gay and bright, and the next group includes playful, graceful, and even quaint and fanciful. Even though most of the remaining adjectives do not discriminate reliably, the differences which they bring out are always in the expected direction. The one possible exception is the adjective soothing, which gives a negligible differentiation, although it has been considered by musicians as characteristic of the minor.

(2) On the second hearing of these 10 musical compositions the *O*s were given a schedule containing a list of 32 adjectives arranged in pairs of opposites, such as merry-melancholy, sad-happy, exciting-soothing, etc., and the instructions were to the effect that they must choose that adjective from each pair which best described the music. It was pointed out that any one composition might not seem to be either truly merry or outstandingly melancholy, but that the choices they made would indicate merely that the music seemed more inclined to one than to the other. In most cases the students apparently found little difficulty in following these instructions, for the omissions, on the average, amounted to less than 5%. Table III shows the results of these votes. The percentage of votes for the major and minor versions are recorded in columns one and two. Since the students were obliged to choose between the adjectives in each pair, 60% in the major for the first adjective of any pair would mean 40% in the minor for the second adjective of the pair. On first thought it might seem that the simple ratios between these percentages would be the best method for rating the adjectives in this table in the order of their importance for characterizing the two modes. This would mean that 'bright' would have a ratio of 75.8 to 43.3 (or 1.75 to 1.00 majority), for characterizing the major mode, and 'satisfying' a 63.4 to 30.5 (or 2.08 to 1 majority), etc. This method, however, is not advisable because, as we pointed out in connection with Tables I and II, such ratios do not take into consideration the number of persons voting, and too much importance is given to adjectives for which the percentage of total votes cast is at a minimum. For example, from the figures given above, the ratio of major to minor for 'bright' as a *major* characteristic gives a 1.75 to 1.00 majority, but the corresponding ratio of minor to major for 'dark' as a characteristic of the *minor* would be 56.7 to 24.2 (a 2.30 to 1.00 majority), simply be-

cause of the fact that there are fewer people voting for happy and therefore any one vote carries more weight. Under the conditions of the experiment (forcing the students to choose one or the other of the adjectives) these two percentages *must* represent the same fact, and any method which

TABLE III

SHOWING PERCENTAGE OF VOTES FOR THE MAJOR ADJECTIVES FOR MAJOR AND MINOR VERSIONS

Adjectives Major (Minor)	Percent votes		D	High Talent and Training	Low Talent and Training	High Seashore	Low Seashore	High C.A.T.	Low C.A.T
	Major	Minor	P.E.$_{diff.}$						
Satisfying (Longing)	63.4	30.5	23.00	11.08	7.90	9.40	7.41	6.24	9.52
Bright (Dark)	75.8	43.3	22.89	13.37	9.20	10.68	7.90	7.70	8.38
Merry (Melancholy)	67.6	35.1	22.73	15.42	9.00	11.22	5.99	5.54	8.40
Happy (Sad)	70.4	37.4	22.45	12.98	10.13	11.40	7.80	5.60	9.40
Gay (Gloomy)	71.6	40.1	21.43	13.04	8.30	10.03	7.66	7.41	8.00
Exalting (Depressing)	71.1	40.6	20.75	11.58	7.90	9.70	7.29	5.09	9.09
Light (Serious)	68.1	37.9	20.54	12.21	8.69	9.20	6.96	5.35	9.60
Triumphant (Frustrated)	65.6	38.8	18.23	8.60	6.30	6.68	6.87	6.76	5.98
Delicate (Heavy)	75.9	54.8	14.93	4.71	8.10	4.39	7.55	4.88	5.33
Spirited (Dull)	73.9	54.8	13.45	10.13	5.96	5.09	4.30	2.67*	5.14
Lyrical (Dramatic)	66.5	50.7	10.75	5.31	3.77*	3.28*	6.76	3.96	2.91*
Vigorous (Tranquil)	50.5	36.6	9.21	8.20	1.70*	5.59	1.12*	1.50*†	5.16
Exciting (Soothing)	50.6	39.1	7.62	5.59	3.96*	4.34	1.50	0.00*	4.20
Secular (Sacred)	74.6	66.8	5.65	3.72*	4.49	4.51	4.54	0.99*†	4.94
Yielding (Obstinate)	58.4	53.1	3.51*	0.38*†	1.87*	1.30*	2.06*	5.35	1.35*
Restless (Quiet)	59.9	55.1	3.18*	6.57	2.45*	3.90*	1.16*	2.67*	1.85*
Median				9.36 ± .89	7.10 ± .57	6.13 ± .56	6.81 ± .54	5.22 ± .39	5.65 ± .52

* Ratios not statistically reliable.

† Indicates that the majority is in favor of the minor adjective.

introduces a discrepancy in the majorities for bright and dark cannot be used in treating these data. In determining the relative importance of these adjectives, we have therefore repeated the procedure used in Tables I and II, and have marked the ratios not statistically reliable with an asterisk. In arranging this table, the percentages recorded are those in which the larger number of votes were cast for the major version; hence this list of

adjectives represents the characteristics of the major mode as they were perceived by this group of *Os*. The associated adjectives, characteristic of the minor, also appear in the parentheses and these two lists of adjectives, with the substantial majorities shown in the ratios in column 3, show again a striking adherence to the historically affirmed characteristics of the two modes.

Since Heinlein, with his method for studying the characteristics of the two modes, found that "any fixity of feeling tone in relation to a given mode is dependent on training to react in a specific manner to a purely intellectual discrimination,"[9] a further analysis of the data was made in order to study the effects of musical training, of musical talent, and of intelligence on the perception of the characteristics of the two modes. Since the 205 *Os* for the experiment were all students in the Laboratory Psychology course, their ratings on scales for music talent and training,[10] their average percentile rankings on the six Seashore tests for musical talent,[11] and their scores on the Minnesota College Ability Tests, were available. The 25 *Os* with the highest scores on the Seashore tests and the twenty-five with the lowest scores, each group representing approximately an eighth of the total group, were singled out; their votes were tabulated separately, and their percentages, and the ratios of major to minor votes, were calculated as above. Likewise the highest and the lowest eighth of these *Os* on the musical talent and training scales, and the highest and lowest eighth on the College Ability Test were separated from the rest of the group and their votes tabulated. The effect of these three factors on the perception of the major and minor characteristics may thus be studied by comparing the ratios for these highest and lowest heights. In Table III, columns 4 and 5, are the ratios for the lowest and highest group in the rating scales for musical talent and training. The actual percentages from which these ratios were calculated are not given, but all that are not statistically significant are marked with an asterisk. While it is true that the ratios for the group with a large amount of training are somewhat larger than those for the group without training, it is also apparent that the students without training do perceive the differences, and to a highly satisfactory and thoroughly reliable degree. On the average the ratios of the highly trained are only slightly better. The median of the ratios for the high group is 9.36 ± 0.89 and for the low group 7.10 ± 0.57. The ratio of this difference to its P.E. (2.26 ± 1.10) does not allow us to predict that there is a real difference

[9] *Op. cit.*, 140.
[10] W. S. Foster and M. A. Tinker, *Notes for Instructors, Experiments in Psychology*, rev., ed., 1929, 31.
[11] C. E. Seashore, *The Psychology of Musical Talent*, 1919, 1-288.

between these two groups. This lack of a substantial difference is the more important in view of the great differences in musical training which these groups represent. The median score in the highly trained group is 90 ± 3.5, while the median for the group with little training is 15 ± 0.7. An examination of the actual papers of the two *O*s who represent the median for each group will give some indication of the nature of the difference in training. The low group median student has never had any private music lessons of any sort, does not play any musical instrument, has no musicians in his family, and never attends operas or concerts. He can read music a little for singing, has sung two years in a High School chorus, and has a piano and a radio in his home. The high group median subject has had private lessons on the piano for two years, voice four years and cello four years, has played four years in orchestras and sung six years in choirs, has three musicians in his family, can read music and provide missing parts, attends concerts very frequently, and can sing or play before small audiences. Yet even with such great differences as these in background and training, the differences in the ability of these *O*s to perceive the characteristics of the major and minor modes are not large, and, what is more important, the students without training do discriminate accurately and reliably between the two modes. There are likewise large differences in the average Seashore percentile scores for the two groups compared in columns 6 and 7. The median for the high group on the six Seashore tests is at the 88th percentile (88.3 ± 0.92) and for the low group at the 40th percentile (40.3 ± 2.01). But the effect of these differences in musical ability on the size of the ratios is very small. There are three non-significant differences for the highly talented group and four for the low group. The median ratio for the high group is 6.13 ± 0.56 and for the low group 6.8 ± 0.54, and this difference of 0.68 in favor of the low group is not even as large as its deviation, 0.78. The ratio is 68/78. This indicates practically no difference in ability to discriminate between the two modes.

Finally, in comparing the lowest and highest eighth of the group in College Ability percentile ratings, we find there is a median of 13 ± 1.6 for the low group, and 95 ± 0.67 for the high group, but in spite of this very large difference in intelligence test scores, the difference between the averages for the ratios is again negligible. The difference is .43 in favor of the less intelligent group, and the deviation is 0.65. The intelligence of the listeners does not affect their ability to discriminate between the modes in any reliable or measurable way.

A comparison, then, of these three factors shows that training is of more importance than either musical ability as measured by the Seashore Tests, or intelligence in discriminating the characteristics of the two modes, but

TABLE IV

ANALYSES OF THE COMPOSITIONS

Composition	$\frac{D}{P.E._{diff.}}$	% of strong beats with thirds	Modulation	Structure, Length	Tonality and Harmony
Martini *Gavotte*	14.58	75	None	F major; allegro moderato; 8 measures 4/4. 4 measure phrase repeated.	Mostly I to V. IV suggested once but only as a passing tone and with the fifth omitted.
Bach *Musette*	10.92	75	None	G major; L'istesso; 16 measures 4/4. ABA form; B preserves same rhythm, similar harmony and melody.	Written in two parts and organ point. Harmony tends toward vagueness because (1) many secondary chords are used; (2) many chords are ambiguous and; (3) many non-harmonic tones fall on accents.
Rameau *Rigaudon*	10.85	56	None	E major; allegretto; 16 measures 4/4. ABA form; A consists of a four measure subject repeated. B has similar rhythm and a subject similar in feeling.	Harmony is mostly I–I–IV V–etc. In B section II_7 and V_7 occur once.
Rameau *Tambourine*	9.58	32	None	E minor; allegro molto 44 measures 4/4. ABACA form. Subjects very similar, staccato and grace notes used in same way throughout.	Harmony and tonality very definite throughout. I and IV alternation throughout except for one transitional passage. Leading tone occurs frequently.
Schumann *Folk Song*	8.88	29	Momentarily to V at phrase endings.	D major; andante; 24 measures 4/4. ABA form. B differs in rhythmic figure, tempo, and type of melody.	In A section, I, IV and V prevail with VI occurring twice and an unusual cadence, VI, II_7, III, I. Section B alternates I–V_7, then modulates to V with a V_7–I in that key.
Beethoven *Sonata*	5.73	55	None	G major; Tempo di minuetto; 20 measures 3/4. A, a short transition, B, and A repeated.	A section alternates I to V. B section somewhat vague for 3 measures when there is an altered d followed by IV–III.

TABLE IV (*Continued*)

Composition	$\frac{D}{P.E._{diff.}}$	% of strong beats with thirds	Modulation	Structure, Length	Tonality and Harmony
Bach *Minuet*	5.05	25	To relative minor	G major; animato; 24 measures 3/4. ABA form. A has two contrasting phrases repeated. B maintains the rhythm of A but the descending figures in the melody contrast wih the ascending figures in a.	In A, I–V prevails with a few IV chords. In B, modulation to relative minor alternating I–V_7. Harmony very clear throughout; only one secondary chord occurs.
Durand *Chaconne*	3.76	53	None	A minor; allegretto; 17 measures 2/4. One subject with two contrasting phrases, repeated	I–V successions with IV occurring at start of each phrase and final cadence IV–V–I.
Gluck *Andante (Orpheo)*	3.65	36	To V	F major; andantino; 28 measures 3/4. ABA form. 8 measures of a; B merely a development of A.	I–IV–I–V. Modulation to V. No secondary chords used except in cadence II_6–I_4^6–V_7–I.
Arensky *Chanson Russe*	1.72	33	To IV and then to relative minor	E minor; adagio; 8 measures 4/4. One subject with two phrases, repeated and in repetition very different harmony with two modulations.	Very complex, many 7ths, appogiaturos and two modulations. I–VI–VII_7–V–I–VI–VII_7–I–VI_9 V_7–I–(a minor) V_7–I_9 (g major) V_7–IV–VII_{13}–VII_7 (e minor) I_4^6–VI_6–V_7–I.

that neither lack of training nor lack of musical ability nor lack of intelligence can obliterate the ability to discriminate the accepted meanings for the two modes.

An attempt was made to discover the particular qualities in the music which account for the differences in the effects in the two modes. In order to make this study, the results were tabulated separately for each composition to determine which item showed large differences and which showed small or unreliable differences. Then an analysis of the music was made in order to account if possible for these differences. For the tabulation, six pairs of adjectives were selected, satisfying, merry, happy, gay, delicate, and exalting, and the ratios for all six adjectives were averaged. Therefore in Table IV we have, not the discriminative ability of any one adjective, on the basis of ten compositions, as in Tables I–III, but the discriminative importance of one composition in terms of these six different adjectives.

Table IV lists the compositions in the order of their importance in differentiating the two modes, and gives also the most important facts about the length, tempo, complexity, and harmonization of the music. It gives also an indication of the number of times the third of the scale occurs. Since the third is probably the most important tone in establishing the minor feeling, the number of times that the third appeared, and especially the number of times it appeared on a strong beat, might be an important factor in establishing the mood effect. We have, therefore, tabulated in Table IV the proportion of strong-beats-with-thirds to the total number of strong beats in the composition.

A study of Table IV does not reveal any clear-cut structural differences in the compositions corresponding to their importance in differentiating the effects of the two modes. A comparison of the items in the first half of the table with those in the second half shows that there is less modulation among the first 5 and a larger percentage of strong beats carrying the third of the scale. The best 5 compositions also show that simplicity of structure and definiteness and uniformity in tonality and harmonization emphasize the different mood effects. This would also be the logical conclusion, since variety in melody, rhythm, or tempo would increase the range of responses and the amount of overlapping for the major and minor, while vagueness or lack of definite character in the tonality would increase the uncertainty on the part of the *O*s and bring the two modes closer together in their effects.

Summary

(1) The previous methods of investigating the meaning, or mood effect, of the major and minor modes have been unsatisfactory either because (a) they left too many factors, such as rhythm and tempo, uncontrolled, or (b) they presented isolated and disconnected elements (chords) which were not apprehended as "music", or (c) there was no possibility of a cumulative effect.

(2) A new method of investigating the effects of the two modes has been devised in which the material used consists of two versions of the same composition, one written in the major key and one in the minor key.

(3) With this method, all of the historically affirmed characteristics of the two modes have been confirmed, in most cases with very substantial majorities, by 205 listeners, for 10 compositions representing a variety of musical forms.

(4) By the method of group differences, the factors of musical background and training, of musical ability (Seashore tests), and of intelligence, have been found to be influential but not in any case essential in discriminating the mood effects of the two modes.

INDIVIDUAL DIFFERENCES IN CERTAIN ASPECTS OF THE LEVEL OF ASPIRATION

By JEROME D. FRANK, Harvard University

The concept of level of aspiration was introduced into experimental psychology by Hoppe.[1] His study consists of a preliminary "qualitative" investigation of this concept, which he defines as "a person's expectations, goals, or claims on his own future achievement" in a given task.[2] To this statement he has immediately added that "the experience of a performance as a success or failure does not depend alone on its objective goodness, but on whether the level of aspiration appears to be reached or not reached."[3] By means of simple experiments he has been able to indicate some of the more important functional relations of the level of aspiration, and to supply some hints as to the significance of individual differences in its behavior. He has also constructed a preliminary theory on which the following hypothesis is based.

The term *level of aspiration,* as used in this paper, is defined as *the level of future performance in a familiar task which an individual, knowing his level of past performance in that task, explicitly undertakes to reach.* By *level of past performance* is meant *the goodness of the individual's past performance, as he knows it.* It is assumed that the relation of the level of aspiration to the level of past performance at any time depends primarily on the relative strength of the following three needs:[4]

(1) The need to keep the level of aspiration as high as possible, regardless of the level of performance. Taken by itself, this need tends to keep the level of aspiration above the level of past performance.

(2) The need to make the level of aspiration approximate the level of future performance as closely as possible. Under the experimental conditions of this investigation, the operation of this need tends to make the difference between the level of aspiration and the level of past performance approach zero in the long run.

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* Accepted for publication December 12, 1933.

[1] F. Hoppe, Erfolg und Misserfolg, *Psychol. Forsch.,* 14, 1930, 1-62. Cf. also T. Dembo, Der Ärger als dynamisches Problem, *op. cit.,* 15, 1931, 50-56.

[2] Hoppe, *op. cit.,* 10.

[3] *Ibid.,* 11.

[4] In this article the term *need* is used to signify any tension in the organism producing a psychological event, regardless of whether the tension is physiological or psychological, innate or acquired, permanent or transient. It is assumed that needs can vary in strength and in quality. Cf. K. Lewin, *Vorsatz Wille, und Bedürfnis,* 1926, 21-29.

(3) The need to avoid failure, where failure is defined as a level of performance below the level of aspiration, regardless of its absolute goodness. This need tends to drive the level of aspiration below the level of past performance.

It seems probable that the relative strengths of these three needs depend not only on environmental factors but also on factors inherent in the individual. In accordance with this view, the height of the level of aspiration relative to the level of past performance, as determined by these needs, would, to an appreciable degree, vary in a consistent manner among individuals regardless of the situation. The person who habitually "keeps his feet on the ground" would be expected to keep his level of aspiration close to his level of past performance, while he whose "head is in the clouds" would presumably send his level of aspiration soaring in any situation. The "cautious" individual would tend, as a rule, to keep his level of aspiration below his level of past performance; the "ambitious" person would typically set his level of aspiration high and persist until he had raised his level of performance to meet it. In short, it may be assumed that certain types of behavior of the level of aspiration express what are usually termed *personality traits*. The purpose of this investigation is to examine this assumption by discovering whether individual differences in the level of aspiration (1) persist through time, and (2) manifest themselves unchanged through a variety of situations.

Method and procedure. In this investigation three groups of *S*s were used, each consisting of 12 individuals.

Two of the groups (referred to henceforth as Groups II and III) consisted of Freshmen and Sophomores in the Summer Session of the College of the City of New York, who gave their time voluntarily. The remaining group (henceforth referred to as Group I) consisted of one C. C. N. Y. student, and 11 students in the Graduate School of Harvard University who were paid for their time.[5]

Three tasks were used, each consisting of some simple performance which was repeatedly carried out. The level of performance was measured in terms of a quantitative scale. At the same time, all other dimensions in which the achievement might vary were eliminated as far as possible through the nature of the tasks and the instructions. A brief description of the three tasks follows:

[5] The writer expresses his thanks to the following people, without whose cooperation these experiments could not have been carried out: to Dr. H. A. Murray of the Harvard Psychological Clinic through whose kindness the Harvard Students were obtained; to the President, the Director of the Summer Session, and other members of the Staff of the College of the City of New York, who generously placed the facilities of the Institution at the writer's disposal; and finally, to the twenty-five students of the College who volunteered to take part in these experiments. The writer also expresses his gratitude to Dr. J. G. Beebe-Center and Prof. G. W. Allport, both of Harvard University, for advice in the development of the experimental technique and evaluation of the results.

The first (Printing) was a motor task with a performance scale of speed. The *S* was required to print standardized words with a linoleum printing set as rapidly as possible. His level of performance was given in the number of seconds required to print the words. The instructions emphasized that speed of printing was all that mattered; neatness would not be considered in the evaluation of the results.

The second (Spatial Relations) was an intellectual task with a performance scale of speed. This task consisted of sections of the Kelley Manipulation of Spatial Relationships Speed Test[6] standardized as to difficulty. The *S* was required to analyze certain spatial relations of a series of figures as rapidly as possible. He was told to work as fast as was consistent with accuracy. It was emphasized that speed was more important than correctness. The *S*'s level of performance was given in the number of seconds required to complete a strip..

The third (Quoits) was a motor task with a performance scale of accuracy. Twenty rings were used. On each trial *S* pitched all 20 from a fixed distance. The measure of his level of performance was the number of rings out of the 20 which he succeeded in getting on the stick. The *S* was allowed to take as long as he wished on each throw, and it was made clear that speed played no part in the evaluation of his achievement.

The tasks were grouped in the following three pairs with respect to the type of ability they required and the nature of the performance scale: Printing and Spatial Relations, Printing and Quoits, and Spatial Relations and Quoits. The tasks of the first pair have the common performance scale of speed but differ in regard to the type of ability they require; the tasks of the second pair both involve motor ability but differ in regard to the nature of the performance scale; the tasks of the third pair, finally, differ both in regard to the type of ability required and the nature of the performance scale.

The first pair of tasks was given to Group I, the second pair to Group II, and the third pair to Group III.

In all the experiments the *S*s were taken singly. The instructions were imparted conversationally, and the situation was kept as informal as possible. With each task, after the technique had been carefully explained, the *S* was allowed one or two trials. After each of these trials he was told how well he had done in terms of the performance scale. On the basis of these levels of performance he was then asked to state how well he intended to do on the next repetition in terms of the same scale. He was then allowed to carry out this repetition. After its completion he was told how well he had done on it and again asked to state his intended level of performance on the next repetition. This procedure was repeated between 10 and 15 times.[7] The *S*'s statement of his intended level of performance before each repetition was taken to represent his level of aspiration at the time.

There were two experimental sessions with each *S*, not less than a week apart. In each session both tasks of a pair were given, separated by a short pause. In the first session there were 15 repetitions of each task, excluding the trials. In the second session there were 10 or 12 repetitions of each task.

The extent to which a given type of behavior of the level of aspiration represents

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[6] Cf. T. L. Kelley, *Crossroads in the Mind of Man*, 1928, 171 f., 184 f.

[7] In Printing and Spatial Relations, each repetition was carried out on different material, all of which had been carefully standardized as to difficulty.

a constant and consistent 'trait' of the personality is conventionally determined by a study of its reliability and generality. In these experiments the reliability of the behavior of the level of aspiration is determined by correlating the data (evaluated as described below) obtained from first and second sessions in a single task. The extent to which the behavior of the level of aspiration is independent of the particular task in which it was observed (*i.e.* its generality), is determined by correlating the data obtained from the two tasks of each pair.

In accordance with the hypothesis sketched at the beginning of this article, the aspect of the level of aspiration which was chiefly investigated was the average difference between its height and the height of the level of past performance over a series of repetitions. This difference was obtained by subtracting from the average of the individual levels of aspiration the median of the levels of performance which preceded each of them.

A numerical example may make this clearer. If there were fifteen repetitions, on each of which the level of aspiration and level of performance were obtained, the median of the levels of performance on repetitions 5 to 14 was subtracted from the average of the levels of aspiration on repetitions 6 to 15. (The level of aspiration on each repetition was based on the level of performance of the preceding repetition.)

TABLE I

RANK-ORDER CORRELATIONS BETWEEN THE FIRST AND SECOND SESSIONS IN EACH TASK

Group	Correlations between		
	P_1 & P_2	S_1 & S_2	Q_1 & Q_2
I	+.72	+.57	—
II	+.60	—	+.26
III	—	+.75	+.63

The first few repetitions were always omitted in the calculation of this measure in order to eliminate factors introduced through the *Ss*' unfamiliarity with the task. The median of the levels of performance was used in order to minimize the effect of extreme values. These were almost always regarded by the *Ss* as "flukes" and as such had little influence on the level of aspiration.

The degree of reliability of this measure is given in Table I, which contains the rank-order correlations between the scores obtained for each of the tasks in the two sessions. The correlations are calculated for each group separately, hence each is based on twelve individuals. For the sake of simplicity of presentation, the probable errors are omitted from all tables in this article.[8]

The correlations for each task are given in the vertical columns. The roman numerals in the left hand column indicate the groups of *Ss* from which the correlations in the corresponding row were obtained.

The tasks are symbolized in all the tables as follows: P = Printing, S = Spatial Relations, Q = Quoits. The numerical subscripts indicate the session. Thus, in the

[8] With 12 *Ss*, all correlations above approximately ±0.45 are more than 3 times as large as their probable errors.

column headed P_1, P_2 are given the correlations obtained between the scores made on the first and second sessions in Printing, and so on.

Results. Table I shows that the difference between the average level of aspiration and the median level of past performance is relatively stable in time in both Printing and Spatial Relations. As far as these two tasks are concerned, the first criterion of the existence of a trait seems satisfied. Whether this conclusion may be generalized to include Quoits remains open because of the statistically insignificant correlation between sessions in Group II. There are two factors which may account for the lowered reliability of this task. These are, first, that the task had a more pronounced 'play' character than either of the others, and, secondly, the fact that between the two sessions many of the *S*s compared among themselves the levels of performance which they had attained in this task, which they failed to do with the other two tasks. The first of these factors would tend to make the *S*'s attitude unstable, while the second would be expected to change the attitude with regard to Quoits in the second session for some *S*s, thereby lowering the reliability of the measure.

It seems safe to conclude from Table I that the average difference between level of aspiration and level of past performance in a given task represents *a relatively permanent characteristic of the personality,* and that *this permanence can be demonstrated regardless of the type of ability which the task requires.*

The question still remains as to whether this measure depends upon the same trait in each of the tasks—whether we are here dealing with a series of specific, though constant, behaviors, or with a single general trait. The answer to this question is given in Tables II, III, and IV. These contain the rank order correlations between the scores made on the tasks of each pair.

Three groups of correlations are given for each pair of tasks, as follows.

(1) Correlations between the combined scores in each task based on both sessions: $(1 + 2)/2$, $(1 + 2)/2$. These show the extent to which the scores on both tasks of a pair are influenced by the same trait of personality, *i.e.,* the degree of generality of the trait. These correlations, being based on the largest amount of data, are of the most worth statistically.

(2) Correlations between the scores made in the two tasks of a pair in each of the sessions (1, 1; 2, 2). These show the degree of generality of the trait investigated in each session.

(3) Correlations between the scores made in one task in one session and the other task in the other session (1, 2; 2, 1). These correlations yield information as to whether the same general trait is operating in both sessions.

TABLE II

RANK-ORDER CORRELATIONS BETWEEN PRINTING AND SPATIAL RELATIONS (GROUP I)

Tasks Correlated	Correlations
$(P_1+P_2)/2$, $(S_1+S_2)/2$	$+.62^*$
P_1, S_1	$+.50$
P_2, S_2	$+.59$
P_1, S_2	$+.54$
P_2, S_1	$+.65$

TABLE III

RANK-ORDER CORRELATIONS BETWEEN PRINTING AND QUOITS (GROUP II)

Tasks Correlated	Correlations
$(Q_1+Q_2)/2$, $(P_1+P_2)/2$	$+.62^*$
Q_1, P_1	$+.70$
Q_2, P_2	$+.01$
Q_1, P_2	$+.45$
Q_2, P_1	$+.06$

TABLE IV

RANK-ORDER CORRELATIONS BETWEEN QUOITS AND SPATIAL RELATIONS (GROUP III)

Tasks Correlated	Correlations
$(Q_1+Q_2)/2$, $(S_1+S_2)/2$	$+.37^*$
Q_1, S_1	$+.45$
Q_2, S_2	$-.08$
Q_1, S_2	$+.26$
Q_2, S_1	$-.06$

* These correlations, corrected for attenuation by the Spearman formula, take the following values: $(P_1+P_2)/2$, $(S_1+S_2)/2 = +0.97$; $(Q_1+Q_2)/2$, $(P_1+P_2)/2 > +1.00$; $(Q_1+Q_2)/2$, $(S_1+S_2)/2 = +0.55$.

The abbreviations used in these tables are identical with those used in Table I. In the left hand column are indicated the scores which are correlated, and the corresponding correlations are given immediately to the right of them. For example, "P_1, $S_1 + 0.50$" means that the correlation between the scores in Printing and Spatial Relations in the first session was $+0.50$, and so on.

The correlations based on the combined scores, $(1 + 2)/2$, $(1 + 2)/2$, are sufficiently high with regard to the first two pairs of tasks (especially in conjunction with their corrected values) to justify the conclusion that *the measure reveals a single trait which manifests itself both in tasks differing in performance scale and in tasks requiring different types of ability.* Put the other way, the correlations demonstrate the existence of a general trait which manifests itself just as strongly in tasks having only motor ability in common as in tasks having only a speed scale in common. Hence the trait depends neither on the performance scale alone nor on the type of ability alone. When the two tasks of a pair differ in both these respects (Table III), the correlation drops, though it remains positive. This seems to indicate that when the physical situations are very dissimilar, it becomes difficult to demonstrate the existence of a single trait covering both of them. That such a trait may still be present in some individuals is shown by the presence of a correlation which is still clearly positive, as well as through additional evidence adduced below.

It has been contended, on the one hand, that general traits do not exist, and, on the other, that traits depend solely on the personality and are manifested regardless of the situation. The above results support neither of these assertions. They point rather to a middle position; namely, that

the data here studied are influenced by *personal traits which are not exclusively dependent on the physical characteristics of the situation,* though they may be to some extent.

This conclusion may be elaborated and qualified through reference to the remaining correlations.

The correlations based on a single session (1, 1; 2, 2) show that in the first session a trait is operating which is largely independent of the physical nature of the task. In the second session the correlation drops to zero in both pairs of tasks involving Quoits, while in the remaining pair of tasks its value is practically unchanged. In other words, when and only when Quoits is one of the tasks, no general trait is manifested in the second session. This startling fact is both confirmed and elucidated by the remaining correlations.

The positive values of these correlations (1, 2; 2, 1) for every pair except those involving Q_2 show that the general trait here manifested is largely independent not only of the physical nature of the task, but also of the session—that it is a personality trait in the full sense of the word. In the pairs involving Q_2 the correlation is again zero. In conjunction with the zero correlations mentioned above, it seems clear that the general trait has disappeared from Quoits alone in the second session, while it is still present in Printing and Spatial Relations. This is probably due to the comparison of performance scores in Quoits between sessions, mentioned above (p. 123). As a result, the general trait manifested in both tasks in the first session is no longer present for most *Ss* in Q_2 while the original trait still operates in P_2 and S_2.

That there are other traits manifested in this experiment which are to a large extent independent of the physical nature of the tasks is indicated by another treatment of the data. This treatment is the calculation of the *rigidity* of the level of aspiration as determined by the fraction:[9]

$$\frac{\text{number of levels of aspiration}}{\text{number of shifts in level of aspiration}}.$$

As this measure does not yield a sufficient distribution of the results to make correlations based on the separate sessions meaningful, only the combined measures for the two sessions will be here considered.

In order to use the results obtained through this treatment of the data as independent verification, it is necessary to determine the extent to which they correlate with the previous results—the average differences between level of aspiration and level of past performance. The correlations between these two groups of results, based on both sessions, are given in Table V.

In Table V, the vertical columns give the correlations obtained for each of the three groups of *Ss*; the rows give the correlations obtained for each task.

[9] In calculating this fraction, the first few levels of aspiration in each session were omitted. See p. 122 above.

Table V reveals that the two measures, while closely related to each other in Group I, are almost completely independent of each other in Groups II and III. This follows not only from the insignificant correlations but from the fact that in one of the two homogenous groups they are positive, in the other, negative. Hence the results yielded by the rigidity measure in Groups II and III can be used as independent verification of the results obtained with the other measure.

In Table VI are given the rank-order correlations between the rigidity scores, based on both sessions, in the two tasks of each pair.

The abbreviations used in Table VI are identical with those used in previous tables. In the left-hand columns not only the pairs of tasks but also the groups of *Ss* are given.

The rigidity of level of aspiration gives almost the same picture as the difference between average level of aspiration and median level of

TABLE V

RANK-ORDER CORRELATIONS BETWEEN RIGIDITY OF LEVEL OF ASPIRATION, AND DIFFERENCE BETWEEN AVERAGE LEVEL OF ASPIRATION AND MEDIAN LEVEL OF PAST PERFORMANCE

Tasks	Groups		
	I	II	III
$(P_1+P_2)/2$	+.65	+.27	—
$(S_1+S_2)/2$	+.63	—	−.24
$Q_1+Q_2)/2$	—	+.19	−.20

TABLE VI

RANK-ORDER CORRELATIONS BETWEEN THE RIGIDITY OF THE LEVELS OF ASPIRATION IN EACH PAIR OF TASKS

Group	Tasks	Correlations
I	$(P_1+P_2)/2$, $(S_1+S_2)/2$	+.64
II	$(P_1+P_2)/2$, $(Q_1+Q_2)/2$	+.66
III	$(Q_1+Q_2)/2$, $(S_1+S_2)/2$	+.56

past performance, despite statistical independence of the measures in Groups II and III. That is, the trait revealed by the rigidity measure shows about the same degree of generality when either the ability tested or the type of scale are the same for two tasks of a pair; a degree of generality, morever, which is strikingly similar to that obtained with the other measure (see Tables II and III). When both the type of ability and the nature of the performance scale are different in the two tasks, the correlation between the two tasks drops somewhat, but with this measure it remains statistically significant. The statistical results obtained with the first measure here receive confirmation. It seems safe to conclude that there is at least one trait manifested in these experiments which is largely independent of the physical nature of the tasks.

It should be mentioned in conclusion that the traits here under investigation seem to manifest themselves far beyond the experimental situations in many of the *Ss*.

To take a single example, one *S* (used in an experiment not reported here but involving a practically identical procedure) consistently kept his level of aspiration far below his level of past performance. If, in spite of this, his level of performance fell below his level of aspiration on any repetition, he was much upset. In short, he showed an intense fear of failure. It was later found that this was typical of the *S*'s behavior in everyday life as well as in the experimental situation. On one occasion he had faithfully completed the work in a course through an entire semester, only to refuse to take the final examination for fear that he wouldn't do well enough.

Further evidence that we are dealing with traits of great generality is found in a study to be reported elsewhere.[10] In this study a single group of *S*s (Group I of the present paper) were put through a large variety of tests and experiments by different experimenters. One of these tests was a measure of the height of the level of aspiration relative to the height of the level of past performance, as used in the present paper. It was found that the scores obtained on this test correlated up to +0.93 with scores obtained in tests administered by other experimenters, and differing physically in all possible respects from those here described. The conclusion seems unavoidable that *the traits which manifest themselves in the present experiment are largely independent not only of the tasks used, but of the entire experimental situation.*

The general conclusion to be drawn from this study is that there are certain traits of the personality, measured through the behavior of the level of aspiration, which are to a large extent independent of the physical nature of the situations in which they are aroused. Specifically, the relative strengths of the need to keep the level of aspiration high, the need to make the level of aspiration approximate the level of performance as closely as possible, and the need to avoid failure, form constellations which are (1) stable in time (except perhaps in a 'play' situation); (2) not dependent alone on either the performance scale of the task or on the type of ability it requires; and (3) often independent of both these factors. In fact, a particular constellation may extend beyond the experimental situation altogether and reveal itself in situations whose physical nature has nothing in common with it. In more usual terminology, we may conclude that these constellations of needs represent traits which show the properties of both reliability and generality. A subsequent article will deal with the behavior of the level of aspiration considered not as a function of the individual personality but as a function of certain general psychological determinants.

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[10] This study will appear in a book soon to be issued by the Harvard Psychological Clinic.

Summary

The term *level of aspiration* is introduced, and the dependence of its behavior on certain needs briefly discussed. An experimental method for investigating the behavior of the level of aspiration is described. By means of this method an attempt is made to determine the extent to which certain types of behavior of the level of aspiration represent a trait of the personality. It is found that the difference between the average level of aspiration and the median level of past performance is highly reliable and is largely independent of the physical nature of the tasks in which it is determined. The latter result is confirmed with the rigidity of the level of aspiration. A study of individual cases reveals that a particular behavior of the level of aspiration may extend far beyond the experimental situation. It is concluded that certain forms of behavior of the level of aspiration depend upon consistent and general traits of the personality.

A COMPARISON OF THE CONDITIONING OF MUSCULAR RESPONSES WHICH VARY IN THEIR DEGREE OF VOLUNTARY CONTROL

By LINUS W. KLINE and HARRIET M. KOHLER, Skidmore College

The purpose of this investigation was to ascertain what difference, if any, existed in the readiness of reaction-patterns of different degrees of voluntary control to respond to a substitute stimulus. Many of the earlier studies of learning involving responses to substitute stimuli were confined to mechanisms conceded to be more or less remote from voluntary control. Such remoteness was regarded as a factor tending to insure the reliability of the findings. Cason, for example, says that "an effort was made [in his studies] to eliminate voluntary factors as far as possible"[1] because, "much of the work which has been done on the conditioned 'reflex' is open to the criticism that the experiments involved 'voluntary factors' which complicated and in some instances vitiated the conclusions reached."[2] However desirable it may have been at first to make use of the simpler and less consciously controlled mechanisms for studying the problems of learning, there is no apparent reason for invoking the principle of freedom from control as a valid criticism against investigating mechanisms under varying degrees of control. In fact, psychology must include these higher reaction-patterns if it is to make a complete study of all types of learning and, besides, common experience furnishes innumerable examples of conditioned voluntary reactions the formation of which invites careful investigation.[3]

The three following mechanisms involving voluntary control were chosen for this study: (1) raising the chin; (2) lifting the heel; and (3) withdrawing the toe. It was assumed that jerking the chin up and back was a less common response than the reaction of either the heel or toe; and further, that withdrawing the toe was less common than raising the heel. The relatively high degree of freedom of the latter response from voluntary control was assumed on the ground of its constant use in walking, whereas withdrawing the toe occurs only when it is threatened or actually punished. According to these assumptions the amount of voluntary control increases from heel to toe to chin. The problem was to ascertain which of these three reaction-patterns would most readily respond to a substitute stimulus.

* Accepted for publication October 4, 1933.

[1] Hulsey Cason, The conditioned eyelid reaction, *J. Exper. Psychol.*, 5, 1922, 153.

[2] Cason, The conditioned pupillary reaction, *idem.*, 108-145.

[3] Joseph Jastrow, On lapses of consciousness, *Pop. Sci. Mo.*, 47, 1905, 481-502.

Subjects. Seven *Ss*, students in elementary psychology, were used. They were unacquainted with the purpose of the investigation and during the experiment occupied a room separate from the one in which the stimuli controls and recording apparatus were set up. The preliminary work necessary for standardizing procedure was done with 6 *Ss* who were acquainted with work on conditioned reflexes.

Apparatus. The adequate stimulus to all three of the reaction-patterns consisted of an up-thrust of a rapidly vibrating metal bodkin activated by an electromagnet.

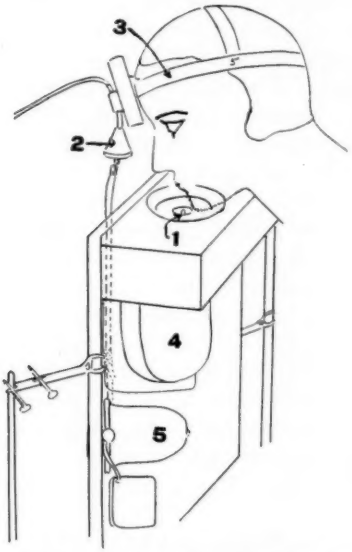

FIG. 1. APPARATUS FOR REGISTERING THE WITHDRAWAL OF THE CHIN

1 = Bodkin in the chin rest; 2 = Tambour by which head movement is recorded; 3 = Headgear; 4 = Electromagnet for vibrating the bodkin; 5 = Electrical bell.

The bodkin for the chin had a blunt end of 2 mm. in diam. and, when vibrating, projected about 1 cm. through a hole in a concave shaped chin rest. The chin was struck at the tip end of the jaw where the flesh is thin. The heel and toe bodkin was drawn to a blunt point and operated by an electromagnet similar to the one used for the chin. The big toe was struck on the volar side near the end; and the heel near the center of the plantar surface. The substitute stimulus was the sound of an electric bell. Both bell and bodkin magnets were attached near together on the same piece of timber and during the preliminary work were activated simultaneously. *S* soon detected the difference in the vibrations of the chin rest and also

of the heel and toe rest when bell and bodkin acted together and when the bell sounded alone. As a consequence the reduced vibrations of the bell alone served as a signal that no punishment would occur. It was, therefore, decided to provide a short time interval, from 0.2–0.4 sec., between ringing the bell and vibrating the bodkin. This was done by using a multiple circuit key and providing the bell and bodkin each with its own circuit. Then the vibrations of the bell magnet were the same whether followed by the vibration of the bodkin magnet or not. Kymographic records were made of all responses.

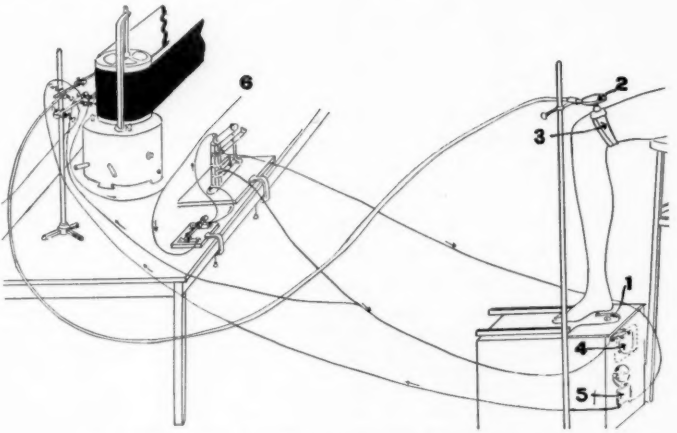

FIG. 2. APPARATUS FOR REGISTERING THE RAISING OF THE HEEL
1 = Bodkin in the heel rest; 2 = Tambour by which raising of heel is recorded; 3 = Cork strapped to leg; 4 = Electromagnet for vibrating the bodkin; 5 = Electrical bell; 6 = Recording apparatus in separate room.

Records of the movement of the chin were secured by the use of receiving and recording tambours (Fig. 1); the former was attached to a head-halter and by means of suitable fastenings activated a recording tambour whenever the head was moved up and back.

Records of raising the heel were secured by a similar device (Fig. 2). A large receiving tambour, 6.5 cm. in diam., was moved by a cork fitted in an upright position just above the knee. The contact between cork and tambour was transmitted by rubber tubing to a recording tambour in the apparatus room.

Records of withdrawing the big toe were made by a writing stylus operated by a cord running over a system of pulleys and tied to *S*'s ankle (Fig. 3). The right leg was supported in a small swing that moved freely back and forth but when at rest it brought the big toe over the bodkin. After each backward response (escape movement) of the leg, the toe was drawn over the bodkin by means of a spring attached to the ankle.

As there was a possibility of a transfer of the effects of learning from one type of reaction to another, the order of procedure was so arranged that no two *S*s, with

but one exception, learned the responses in the same order. Such a precaution, it was thought, would tend to minimize the effects of transfer. The order used for each *S* is shown in Table I.

Procedure. The time interval between successive stimulations by the bell and bodkin, or by the bell alone, was varied in accordance with one of two plans. Plan A

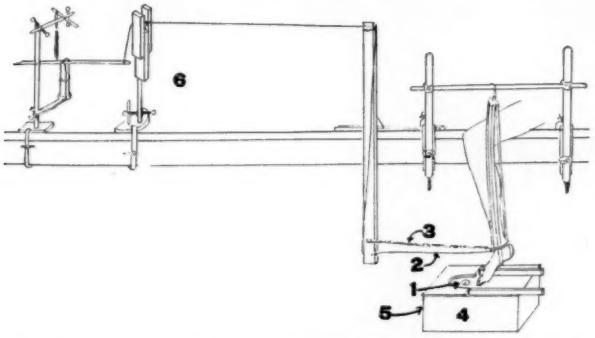

FIG. 3. APPARATUS FOR REGISTERING THE WITHDRAWAL OF THE TOE
1 = Bodkin in toe rest; 2 = Cord by which withdrawal of toe is recorded; 3 = Spring which returns toe to position; 4 = Electromagnet within box for vibrating the bodkin; 5 = Electrical bell on front of box; 6 = Recording apparatus.

called for 88 punishments and for 8 substitute stimulations. The time intervals between successive stimulations varied from 6–20 sec. A period of 23 min. and 4 sec. was required to administer Plan A. Plan B called for 48 punishments, and for 8 substitute stimulations, and the time intervals varied from 6–15 sec. It required 9 min. and 6 sec. to administer Plan B. The short plan applied twice in the same

TABLE I

ORDER OF STIMULATING THE DIFFERENT REACTING SYSTEMS

	Reacting mechanisms		
	First	Second	Third
EE	chin	heel	toe
VM	chin	heel	toe
VB	chin	toe	heel
SC	toe	chin	heel
CL	toe	heel	chin
IN	heel	chin	toe
HT	heel	toe	chin

working period afforded 96 punishments, and seemed to be better suited to refractory *S*s than the long plan used only once. In interpreting the plans the numbers in parentheses indicate that only the bell was rung on the last count of the given number, whereas in the case of the other numbers both bell and bodkin were presented, not simultaneously, but in close succession, as already described. It

should be understood that the success in conditioning *S* depends largely upon the time-interval allowed between the presentation of the bell and bodkin. This time-interval is acquired only by practice, for it must be long enough to preserve the quality of the bell and short enough to maintain the pattern characteristics of the stimulus.

Plan A. 6, 10, 15, 12, 15, 18, 15, 16, 18, 20, 15, 18, 15, 12, 18, 20, 15, 10, 16, 14, 18, 13, 10, 15, 11, 8, 16, 18, 12, 18, (18), 15, 10, 16, 14, 12, 16, 15, 18, 17, 15, 16, 10, 15, 13, 9, 15, 18, 15, 16, 14, (15), 12, 13, 9, 14, 16, 17, 15, 10, 13, 12, 15, 17, 18, 13, (15), 13, 17, 12, 14, 10, 12, 15, 18, 14, (18), 15, 13, 12, 17, 13, 15, 14, 15, (14), 10, 11, 13, (10), 18, 19, 17, (18), 17, (18).

Plan B. 6, 10, 15, 10, (9), 12, 13, 9, 8, 11, 8, (6), 12, 13, 14, 11, 12, 15, 10, (7), 15, 12, 8, 10, 6, 8, 10, (8), 15, 11, 9, 8, 4, 10, (5), 10, 6, 12, 8, 9, 12, 7, 12, 8, 6, (6), 8, 12, 10, 13, 11, 8, (10), 6, 8, (8).

Results. The progress in learning to respond to the substitute stimulus was divided into three stages as follows: First, the *unconditioned* stage in which no conditioned response occurred; secondly, the *partially conditioned* stage having a variable number of conditioned responses; and thirdly, *completely conditioned* stage. An *S* was regarded as conditioned when responding 8 times in succession to the bell alone. A critical study of the three stages by *S*s and by type of reactions sheds additional light on the growing conception of relatedness of apparently diverse neuromuscular reactions. Table II shows that when a response to the bell alone had been fully conditioned, or well established, for one kind of pattern reaction, it was comparatively easy to evoke a conditioned response from either of the other two patterns.

In detail, the results show that *EE* began by reacting with the chin and required 176 punishments before reaching the completely conditioned stage; this was followed by raising the heel which required only 50% as many punishments before being fully conditioned and when withdrawing the toe was started, it was found to be already conditioned. *VB* also began with the chin response and soon gave evidence of being partially conditioned, but required 231 punishments before passing to the complete stage. The toe reactions of this *S* were evoked 3 weeks later and required only 38% as many punishments as the chin before being fully conditioned. On beginning with the heel response 10 days later, it appeared that stable associations with the bell already existed because no failures to respond to the bell alone appeared. *VM* of the chin group, *i.e.,* the group which made the chin reactions first, was sensitive to both bodkin and bell as well shown by exaggerated kymographic records, yet no signs of complete conditioning appeared during the first experimental period. Two days later, however, at the same hour she reached the third stage. Thirty-three days later when raising the heel, full responses to the bell alone appeared at once and continued to the end of the experimental period. The same degree of learning was found in the toe reactions of this *S* two days later.

Before *CL* and *SC* began to follow the time order plan they each spent 4 periods reacting to the chin apparatus receiving 352 punishments and showing a negligible number of conditioned responses—2 and 3 respectively. Both now began reacting

TABLE II

SHOWING RESULTS IN LEARNING DIFFERENT TYPES OF RESPONSES

S	Response	Date	Unconditioned		Partially conditioned			Fully conditioned	
			No. Punishments	Possible Conditioned Responses	No. Punishments	Unconditioned Responses	Conditioned Responses	No. Punishments	Conditioned Responses
	Chin	Mar. 3	88	8					
	"	" 8	50	2	38	4	2		
	"	" 15						88	8
EE	Heel	April 12			88	2	6		
	"	" 13						88	8
	Toe	" 17						88	8
	Totals		138	10	126	6	8	264	24
	Chin	Feb. 27			88	3	5		
	"	" 28			88	5	3		
	"	Mar. 13			55	1	2	33	5
VB	"	" 14						88	8
	Toe	April 7			88	5	3		
	"	" 10						88	8
	Heel	" 20						88	8
	Totals				319	14	13	297	29
	Chin	Mar. 13	88	8					
	"	" 15						88	8
VM	Heel	April 17						88	8
	Toe	" 19						88	8
	Totals		88	8				264	24
	Chin	Mar. 13	88	8					
	"	" 16	88	8					
	"	" 17	88	8					
	"	" 20			88	6	2		
	Totals		264	24	88	6	2		
	Toe	April 7	88	8					
	"	" 10	88	8					
	"	" 11	88	8					
	"	" 12	88	8					
	"	" 13	78	13	18	2	1		
SC	"	" 14		[Plan B]	144	24	0		
	"	" 17			96	16	0		
	"	" 19			96	14	2		
	"	" 24			40	2	4	56	10
	Chin	" 25						48	8
	Heel	" 25						92	15
	Totals		430	45	394	58	7	196	33

TABLE II (*Continued*)

SHOWING RESULTS IN LEARNING DIFFERENT TYPES OF RESPONSES

			Unconditioned		Partially conditioned			Fully conditioned	
S	Response	Date	No. Punishments	Possible Conditioned Responses	No. Punishments	Unconditioned Responses	Conditioned Responses	No. Punishments	Conditioned Responses
	Chin	Mar. 10	88	8					
	"	" 15			88	5	3		
	"	" 16			88	8	0		
	"	" 17			88	8	0		
	Totals		88	8	264	21	3		
	Toe	April 10	88	8					
	"	" 11			88	7	1		
	"	" 12			88	6	2		
CL	"	" 13			96	12	4		
	"	" 14			96	12	4		
	"	" 17			96	10	6		
	"	" 20			88	3	5		
	"	" 21						48	8
	Heel	" 21			96	4	12		
	"	" 24						48	8
	Chin	" 26						48	8
	Totals		88	8	648	54	34	144	24
	Heel	April 24						88	8
IN	Chin	" 25						88	8
	Toe	" 26						88	8
	Totals							264	24
	Heel	April 21						88	8
HT	Toe	" 25						88	8
	Chin	" 26						88	8
	Totals							264	24

with the toe and continued for three or four periods showing only a negligible number of conditioned responses. *CL* after receiving 640 punishments on the toe was fully conditioned. The heel reaction of this *S* was next evoked and required 96 or only 15% as many punishments as the toe to be fully conditioned. Two days later *CL* was placed in the chin apparatus, and to the surprise of the operators, was apparently completely conditioned. Attention has already been called to the fact that this *S* even after 352 punishments of the chin had given no evidence of being conditioned. But as soon as the toe was conditioned, the other two were, or nearly so. The record of *SC* parallels very closely that of *CL*, the former requiring 824 punishments of the toe up to the stage of complete learning. Returning the next day to stimulating the chin, which a month before had received 352 punishments without any evidence of being conditioned, the experimenters now found it to be fully conditioned, and the same was found to be true of the heel.

Thus far the heel had not been the first to be stimulated and conditioned. In the order of procedure it had always come second or third for the *Ss*, that is, after one or both of the other two had been fully conditioned. For that reason, in part, its comparative readiness to respond to substitute stimulus was about the same as the toe and chin. But with subjects *IN* and *HT* the heel came first and in both cases it responded to the bell alone at the first opportunity, that is, after 30 punishments according to Plan A. With the data at hand only a tentative account of the readiness of these two *Ss* to respond to the bell can be given.

Discussion

The object of the present investigation is realized. Of the three mechanisms studied, the heel-leg is the most susceptible to conditioning. This result might be explained on the grounds that the nerves and muscles for lifting the heel form a part of the walking and dancing movements and that both of these activities readly respond to sound stimuli in ordinary life. This explanation does not, however, account for the results obtained with *EE* and *CL*, who required a period of heel punishment even though they were already conditioned at chin and toe.

IN and *HT*, although having normal emotional scores and being otherwise apparently normal, maintained a jumpy, skittish, and apprehensive attitude toward the apparatus throughout the experiment. Their unusual results may be due to an individual attitude of a super-sensitive type. That individuality was an important factor in producing these results is further substantiated by comparing the differences in the number of punishments required among the other *Ss* to secure complete learning. *SC* and *CL* being the first conditioned in the toe response required the highest number of punishments which would seem to express a refractory disposition. Again, the medium number of punishments required by *EE* and *VB* express a neutral or complacent attitude. Among the 7 *Ss* that served throughout the experiment there appeared three sorts of individuality: super-sensitive; complacent or indifferent; and refractory.

The results, with the possible exception made above for the heel, fail to show any connection between an assumed amount of voluntary control over a mechanism and its readiness to react to an inadequate stimulus. They do show, however, that when any one of the three mechanisms is conditioned to a substitute stimulus, the other two are likewise ready to react to it.

The sensation pattern aroused by the bell was, of course, the same in all cases, but the sensation patterns of the adequate stimulus, the bodkin, were somewhat changed; first, with the change in the locus of application on the person, and secondly, by the difference in the striking surfaces of the

bodkins—the chin bodkin with a surface of 2 mm. in diam. gave a hammer-like blow, and the toe-heel bodkins gave a jab. For these reasons the combined sensation-patterns were slightly altered with, but not by, the change in the reaction-patterns. The amount of difference between each of the three reaction mechanisms can not be satisfactorily given because of the difficulties involved in describing muscular movements; but it is quite obvious that a decided difference both in structure and function exists between the dorsal muscles of the neck that move the head and chin and the gross muscles of the leg that lift the heel and withdraw the toe. While it is clear that no such difference exists between heel-lifting and toe-withdrawing—doubtless there are many muscles common to both movements—yet there exists both in quality and in quantity a considerable difference based upon the differences in the neural and muscular combinations involved in making the two movements. Hence it is safe to say that the three responses differ each from the other but not in equal amounts. It appears, then, from these observations that the receptors and sensation-patterns were the same, barring the exceptions noted, and that the three reaction mechanisms differed from each other in variable amounts. The next question in order concerns the nature of the cortical connections between the sensory-patterns and each of the three reaction-mechanisms.

The more generally accepted notion of the cortical connections based upon the reflex conduction or brain path theory would claim that there is provided in the cortex three separate neural systems to transmit the nerve currents from each of the slightly different sensory patterns to each of the quite different effectors, or as recently stated by Dunlap, "if the neural chain theory be true, there should be set aside for certain responses certain neuron chains and for other responses other neuron chains."[4] The findings detailed above fail to support such a theory implying as it does a high degree of specificity in the cortical neurons, but instead they tend to show with some emphasis that one and the same cortical pattern may function for several different reaction systems or that many of the integrated brain neurons control many diverse responses. Much evidence has been accumulated in recent studies tending to show that the cortical regions of the brain function as a unit.[5] Human infants make general and total responses to specific stimulations. Allport and Vernon have amassed evidence tending to show that the unitary responses of infancy persist into adult life and thereby "maintain a temperamental" consistency in person-

[4] Knight Dunlap, *Habits: Their Making and Unmaking*, 1932, 53.
[5] C. E. Coghill, *Anatomy and the Behavior Problem*, 1929, 87 ff.

ality and that this is done in spite of the "increased specialization in skill."[6] The three reaction-patterns under investigation are of the fundamental type according to Mercier,[7] and as such they have no need for neural specialization in their control because they are still under the control of the mass action of the brain.

The conception that the cortex functions as a unit or that one and the same system of cortical neurons controls diverse modes of reaction makes the process of transfer both unnecessary and erroneous as an explanatory principle of the rate of this type of learning. Doubtless where 'accessory' reaction-patterns are involved in learning, as in the voice or in the hand, the principle of transfer operates. Lashley points out "that the rate of learning is not dependent upon the properties of individual cells, but is somehow a function of the total mass of tissue," and that "somehow the motor system must be sensitized to respond to the sensory patterns, but the phenomena cannot be expressed in terms of definite anatomical connections."[8] This view of the unitary mass action of the brain in its associational functions may be used to interpret the results of the present study, for we may suppose that such action of the brain involved in training one of the responding mechanisms, functioned in a similar capacity during the training of the other two, and for that reason the other two were found to be practically trained as soon as they began to react to the conditions of the experiment—there was no transfer because the same central reaction-patterns were used in all three responses.

[6] G. W. Allport and P. E. Vernon, *Studies in Expressive Movement*, 1933, 18 *f.*
[7] Charles Mercier, *The Nervous System and the Mind*, 1888, Chap. IV, 81-98.
[8] K. S. Lashley, Basic neural mechanisms in behavior, *Psychol. Rev.*, 37, 1930, 8.

SOME CHARACTERISTICS OF RHYTHMIC PERCEPTION

By F. H. Lewis, Bates College

In connection with research on some of the conditions for auditory grouping, the question arose as to whether there are any well defined rhythmic preferences. Simply stated, the problem turned out to be that of the relative affective value of 2/4 rhythms compared to that of 3/4 rhythms. One might state the matter popularly, *e.g.* whether people prefer fox-trot or waltz music, in so far as the rhythmic factor is concerned. The popular statement is, however, not nearly so simple and straightforward as it seems, particularly where one attempts to establish experimental evidence for the preference. The difficulties are essentially the same as those encountered in the study of the affective value of colors. One of these difficulties, certainly, is that of controlling the associative or meaning factor. Using single tones, the meaning or associations of musical notes are probably less insistent and more easily regulated than those of simple colors; but in the ordinary situation involving enough factors to produce rhythm one is confronted with the following alternatives: (1) either of abandoning the effort to control the meaning and simply taking the data at their face value; or (2) of limiting the situation so that the data however consistent do not mean anything except with respect to the particular and individual conditions. In either case, generalization is unwarranted. Either of them, however, particularly the second, should add to our knowledge of the psychology of rhythm.

Some of the factors for producing rhythms ordinarily encountered are timing, intensive differences or accent, tempo, quality (which includes not only timbre with all of its ramifications, but also pitch differences), melodic form, and the little known but highly suggestive factors of grouping such as common fate, closure, and so-called 'goodness.' The variable physical rhythm-producing factors were brought down to two, for the purposes of the experiment: temporal arrangement and accent. Less complete limitation of the conditions, brings in a strong subjective factor in the form of associations; greater reduction as, for instance, the use of timing alone, results in the appearance of another type of subjective factor which shall be mentioned below.

The apparatus producing the rhythmic patterns was a 1000 cycle oscillator, whose tones were amplified and timed by a kymograph. Wires running from two of the posts of the oscillator to spring-contacts against the kymograph drum provided accent by differences in intensity, and records cut from paper and pasted to the kymograph drum gave the different groupings. The *O*s sat in front of the loud speaker in another room from that in which the rest of the apparatus was set up. Nine rhythms were used: 4 fox-trot (2/4), 4 waltz (3/4), and, for control, an unaccented series of tones regularly timed and uniformly loud which we shall refer to as the 'straight' rhythm. If it be asked why only 2/4 and 3/4 patterns were used, the answer is that patterns of five, seven, eleven and the like do not form very stable groups, breaking up readily into the more fundamental patterns. Needless to say, groups of four, six, eight, nine and other multiples of two and three are simply special forms of the so-called fox-trot and waltz time. The *O*s made their judgments by the method of paired comparisons, each rhythm being compared with each other, making 36

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judgments in all. Since a rhythm takes time to build up in experience, it is not something which can simply be handed to a person all of a sudden, then another and simultaneous judgment secured. It was necessary, therefore, to present each rhythm several times, or for a complete revolution of the kymograph, make a pause of about 5 sec. then present the comparison series similarly.

A preliminary set of observations taken with three classes in General Psychology brought out a 6 to 5 preference for 3/4 rhythms.[1] This was a surprising verdict in view of the fact that a question proposed previous to the trials yielded an 8 to 1 preference for fox-trot as opposed to waltz music in general.

Attacking the problem more systematically, 6 *O*s, all college students, interested but not professionally trained in music, made the comparisons 4 times over with a 24-hour interval between series. Table I is a condensation of the individual data.

TABLE I

SHOWING THE NUMBER OF TIMES THAT THE DIFFERENT RHYTHMS WERE PREFERRED

Order	Rhythm		
	3/4	2/4	"straight"
Original	83	26	11
Reversed	85	54	5
Original	80	41	23
Reversed	98	42	4
Total	346	163	43

The most obvious thing about the figures is the greater than 2 to 1 preference for the 3/4 rhythms. But the relatively large number of 'straight' rhythms chosen is also interesting, especially since there were four times as many 2/4 and 3/4 rhythms as 'straight.' It is worthy of note that in none of the introspections made after the fourth sitting with instructions for complete introspection, is any mention made of the 'straight' rhythm. On questioning one of the *O*s, a normally intelligent and musical person, it was found that he had not even noticed the unaccented rhythm. When pressed for a more careful report of his experience he said that he was, in fact, bothered occasionally by a difficulty in getting a rhythm defined for himself, but that not knowing the exact nature of the experiment, he reasoned that it might have something to do with rhythmic limens, and so dismissed the possibility of an evenly times series. Incidentally, the strength of the subjective element in this case, suggested a group experiment in which the class was told that they were being given a test in rhythmic acuity and that in each pair of rhythms presented they were to judge which was the 2/4 and which the 3/4. The series were all exactly alike, evenly timed. Four-fifths of the answers returned were 2/4 or 3/4.

Turning now to Table II, we find that whereas the simple and complex 3/4 rhythms were about equally preferred, with a slight advantage for the simple ones, the reverse is true with the 2/4 rhythms. Examination of the data sheets showed that the cases where the 2/4 rhythm triumphed over the 3/4, it was the complex 2/4 rhythms which were effective in so doing; the simple 2/4 rhythms uniformly

[1] Credit is due Mr. G. E. Stevens for collecting the data which follows.

losing when compared with the 3/4. The introspections throw no light on this result.

The introspections did, however, bring out a point which has been prominent in previous studies of rhythm. It concerns the presence and development of imagery, particularly visual imagery. It is significant not only that these comments came from the *O*s unsolicited, but also that they came from persons who knew neither the exact purpose of the experiment nor the experimental literature on the subject. Ruckmich mentions the fact that in the course of an experiment on visual rhythm, the kinaesthetic factor recedes into the background.[2] It is interesting to note, therefore, that in the course of an experiment on auditory rhythm (which for our *O*s was largely kinaesthetic) the visual imagery should tend to recede into the background. This might be due to a growing feeling of monotony. The visual imagery mentioned included such things as ballet dances, symphony orchestras in action, Paderewski at the piano, and even an image of *E* shifting records. But according to two of the protocols,

TABLE II

SHOWING THE NUMBER OF TIMES THAT THE DIFFERENT RHYTHMS WERE PREFERRED

Rhythm			
3/4		2/4	
Sim.	Com.	Sim.	Com.
24	6	15	15
15	20	8	28
19	17	14	26
17	19	11	25
—	—	—	—
75	62	48	94

this visual accompaniment disappeared toward the end, and as the choices became more mechanical, the visual, as well as the auditory imagery gave way to a more conceptual type of judgment.

The time necessary for presenting the two members of the pairs of rhythms makes it impossible to fit the data into the usual expectancy of time-errors. If two stimuli are separated by an interval of 2 sec. or more, there is a tendency for the second stimulus to be preferred, ceteris paribus. If the interval is less than 2 sec., the first tends to be preferred.[3] Since the interval in this experiment always exceeded 2 sec., it might be expected that the influence would work in a negative direction, but by the statistical and introspective evidence the thing seems to go the other way. The percentage of change of preference was about 20 with almost all of the changes being made in favor of the first member. The following introspection brings out this point. "Unless it was very unpleasant, or followed by a much better one, the first one seemed better. Especially a second one which was unpleasant made one imagine that the first one must have been better." It is significant, also that the strongest showing of the 'straight' rhythm is to be found in Sittings I and III, at which time the 'straight' was always given before any other rhythm presented with it. Since cer-

[2] C. A. Ruckmich, Visual rhythm, *Titchener Com. Vol.*, 1917, 253.
[3] J. G. Beebe-Center, *Pleasantness and Unpleasantness*, 1933, 197-198.

tain rhythms, like the simple 2/4 with simple 3/4 or the 'straight' with almost any of the 3/4 rhythms maintained a strong constancy, a statement concerning the time-order factor should be as follows. In cases where there is some equivocality of judgment, the first member of the pair is favored. How far this is a matter of mood or immediate circumstances, it is not safe to say. The following introspection related to this factor of temporary mental set shows how immediate surroundings may affect the judgment. "If I noticed something pleasant, such as Virginia's eyelashes, I liked better the rhythm in which I noticed them." The question of the time-error in rhythms is a legitimate one and should have more specific study.

Since the whole question of rhythm is so bound up with the facts of kinaesthesis, it was decided to use the set-up to discover which, of the two principal patterns, was the greater inducer of movement. Automatograph records obtained when the *O*s were listening to the two rhythms showed greater activity both in strength of movement and distance covered, for the 2/4 rhythms. No significant introspections could be secured on this phenomenon.

The question arises as to the social influence on preferences of this sort. To be sure, the popular preference will vary *in general* with the times, and *in particular* with individual pieces of music so that it is doubtful whether the experimental psychologist can generalize on rhythmic preference, even as a social phenomenon. But where specific and measured factors are used to set up rhythms whose principal feature is the actual rhythm itself, the uniformity of results which can be obtained would make it appear that the affective psychology of rhythm is probably in the same experimental class with that of colors, illuminations, odors, geometrical forms and similar fields of investigation. Whether the introduction of other factors would change the principal finding of this report; namely, that under the conditions mentioned, 3/4 is preferred to 2/4 time, is a matter of further study.

WORD-ASSOCIATION REACTION-TIME AND THE MAGNITUDE OF THE GALVANIC SKIN RESPONSE

By WILLIAM A. HUNT, Connecticut College for Women, and CARNEY LANDIS, New York State Psychiatric Institute

The relation between an individual's reaction-time and the magnitude of his galvanic skin response to word-association stimuli has long been a matter of interest. Since both are often used as complex indicators in the word-association technique,[1] one might expect to find them in close agreement. The evidence in the literature is somewhat equivocal. Thus Peterson and Jung, in an early paper, state tentatively that "in certain cases there is a clear parallelism between the length of reaction-time and the height of the galvanometer curve."[2] Recently Hathaway has reported a clear correlation of 0.60 ± .086.[3] Crosland fails to confirm this finding, reporting values that "range from —0.195 to +0.116 with large probable errors."[4] Unfortunately certain flaws in Crosland's technique raise some doubt as to the reliability of his results.[5] Hathaway's apparatus has also been questioned as an instrument for research.[6] As a result, further work on the matter would seem valuable.

The material to be reported on here was obtained during an extensive investigation of the conscious correlates of the galvanic skin response which will be published later. The writers do not intend in the present paper to deal with the general subject of complex indicators, or with the validity of either reaction-time or galvanic reflex magnitude as such an indicator. It is their intention to limit themselves solely to the relation existing between reaction-time and the magnitude of the galvanic skin response. In the work bearing on this particular point 22 *Ss* were used: 14 Normals, 4 cases of Dementia Praecox, and 4 Manic Depressives. Two different series of association words were used during two distinct experimental hours on separate days. The first series consisted of 20 English words arbitrarily chosen as a normal sample by the experimenters. The second series contained 30 words chosen at random from the Jung list. An improved Wheatstone bridge of the sort used by Darrow,[7] together with a Leeds and Northrup reflecting galvanometer was used to register the galvanic deflections. The deflections were followed by hand with a sliding pointer on a scale

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[1] C. G. Jung, *Studies in Word-Association*, 1918, 452-530.

[2] F. Peterson and C. G. Jung, Psychophysical investigations with the galvanometer and pneumograph in normal and insane individuals, *Brain*, 30, 1907, 211.

[3] S. R. Hathaway, A comparative study of the psychogalvanic and association time measures: A new psychogalvanic apparatus, *J. Appl. Psychol.*, 13, 1929, 632-646.

[4] H. R. Crosland, Measurements of emotion by a method which combines the word-association, reaction-time technique with the psychogalvanic technique, *Psychol. Bull.*, 28, 1931, 575; and H. R. Crosland, and L. F. Beck, Objective measurements of emotion, *Univ. Ore. Publ., Psychol. Series*, 1, 1931 (no. 3), 129-202.

[5] T. W. Forbes, and C. Landis, A critique of Crosland's "Measurement of Emotion," this JOURNAL, 45, 1933, 522-526.

[6] T. W. Forbes, The Hathaway galvanic apparatus as an instrument of research, this JOURNAL, 45, 1933, 502-511.

[7] C. W. Darrow, The Behavior Research Photopolygraph, *J. Gen. Psychol.*, 7, 1932, 215-219.

2 ft. from the galvanometer mirror. This in turn was connected by a reducing pulley to a Weiss-Renshaw polygraph. The actual deflections were read off from the polygraph record in 100 ohm units. Stimulus-words were presented verbally and reaction-times were taken in hundredths of a second with a Meylan stop watch by an *E* sitting at the right rear of the *S*. The recording apparatus was in a separate room under the care of a second *E*. When a deflection followed a stimulus-word no further stimulus-word was given until *S*'s resistance either returned to the previous level or definitely established itself at a new level. Thus there was no overlapping of deflections. Correlations were run between the deflection magnitudes and the reaction-times using the product-moment formula for *r*. The results for each on each series of association words are given in the accompanying table.

SHOWING FOR EVERY *S* THE CORRELATION BETWEEN THE REACTION-TIME AND THE MAGNITUDE OF THE GALVANIC SKIN RESPONSE

S	First series		Second series		Diff. between rs
	r	P.E.	*r*	P.E.	
	−.10	.15	−.12	.12	.02
	+.45	.13	−.01	.13	.46
	+.22	.14	+.48	.10	.26
	−.23	.14	+.49	.09	.72
	−.03	.15	+.40	.10	.43
	−.71	.12	+.12	.13	.83
Normal	+.57	.10	+.39	.12	.18
	+.02	.15	+.73	.06	.71
	+.02	.17	+.79	.05	.77
	−.01	.16	+.39	.10	.40
	+.16	.15	+.19	.13	.03
	+.66	.10	+.47	.10	.19
	+.25	.15	+.09	.13	.16
	+.18	.15	+.09	.13	.09
	+.30	.17	+.16	.12	.14
Manic	+.22	.17	+.45	.10	.23
Depressive	−.02	.17	+.41	.11	.43
	+.53	.13	+.01	.13	.52
	+.34	.16	−.16	.13	.50
Dementia	+.53	.12	+.17	.12	.36
Praecox	−.01	.17	+.03	.13	.04
	−.13	.17	+.47	.10	.60

Examination of the table seems to show a definite tendency toward a positive correlation. This tendency, however, seems to be neither strong nor reliable. Since the tendency is slight it does not seem worth while to analyze our data by more refined methods such as expressing each deflection as a percentage of its initial level. Out of 22 *S*s there are only 4 in the first series and 8 in the second whose positive correlations are at least 4 times the probable error. There is even one reliable negative correlation in the first series, although none show in the second series. The fact that only 18% of the *S*s in the first series and 36% in the second show reliable positive correlations indicates that there is *only a tendency* for large galvanic deflections to accompany long reaction-times. Furthermore, it is important to notice that only *one S* gave a correlation 4 times the probable error in *both* stimulus series.

This variability of performance between the two series becomes clearer if we run a rank order correlation between the relative standings of the *Ss* in the two series. The rank order correlation is —0.14 ± .14. This lack of agreement between the two series is also shown by the fifth column of the table which shows the numerical difference between the first and second correlations for each *S*. The writers feel that this variability may explain, through the workings of the sampling error, the previous conflicting results reported in the literature. Since an *S* may apparently show a positive correlation one day and none the next day, this lack of reliability might lead to the chance selection of small groups that would exhibit, on the basis of insufficient testing, either positive or negative results.

Although the small number of *Ss* makes any comparison between the Normal and the Dementia Praecox and Manic Depressive groups indicative rather than conclusive, our limited results would seem to show no differences between these groups with respect to the factors here considered.

In conclusion, then, we may state that while our results seem to indicate some tendency for large galvanic deflections to be accompanied by long reaction-times, the relation is neither close nor constant.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF NORTHWESTERN UNIVERSITY

VI. The Relative Importance of Eye and Hand Dominance in a Pursuit Skill

By G. L. Freeman and J. S. Chapman

It is well known that skills involving eye-hand coördination usually include the use of the dominant hand. The rôle of eye dominance in such performance is not so clearly understood. Lund,[1] who investigated this problem in connection with dart-throwing, reported that the use of the dominant eye resulted in a better score than the use of the non-dominant, and that both eyes used together gave the highest score. Lund's procedure involved a moving hand and a fixed eye. Our problem was to obtain comparable records in a test situation calling for a moving eye as well as a moving hand. To this end we utilized manual pursuit, testing performance of this skill under conditions where dominant eye and hand were used both together and with non-dominant parts.

A group pursuit test. In the course of preliminary investigations we were able to develop a group pursuit test which seemed to possess advantages over the individual pursuitmeter techniques of Miles,[2] Renshaw,[3] and others. Consequently it was used in our study. The test consisted, in essential, of tracing in ink upon cellophane the movement of a dot as seen in a mirror lying below the writing surface. As shown in Fig. 1, a circle was drawn on the floor and the *S*s seated about its circumferance. An ordinary projection lantern was arranged to throw images directly upon the ceiling. The chairs were placed so that all the *S*s saw these images (in their mirrors) from the same angle. The details of the test are best discussed under four headings.

(a) Control of stimulus movement. The stimulus movement was provided in the following manner. A piece of glass twice as long as the slide ordinarily used in the projection machine was placed upon a horizontal carrier platform. The half which lay over the lens had a vertical reference line, the half not exposed had a series of horizontal lines arranged in a zigzag fashion. A long narrow strip of glass carrying two small black dots rested upon the double slide. *E* followed the pattern of horizontal lines with the right-hand dot, and as the glass strip zigzagged back and forth from top to bottom the movement was duplicated upon the ceiling by the visible left-hand dot. *E* controlled the rate of movement by counting.[4]

* Accepted for publication October 2, 1933.

[1] F. H. Lund, The dependence of eye-hand coördinations upon eye dominance, this Journal, 44, 1932, 756-762.

[2] W. R. Miles, The Pursuitmeter, *J. Exper. Psychol.*, 4, 1921, 77-105.

[3] S. Renshaw, An experimental test of the serial character of a case of pursuit learning, *J. Gen. Psychol.*, 1, 1928, 520-533.

[4] It is possible to control both the rate and the direction of movement by attaching the glass glider to machinery. An even simpler way of assuring uniformity in successive presentations is to have a 'movie film' made of the pattern. We found,

(b) Recording the responses. Since the *Ss* were seated with their mirrors equidistant from the center of the circle, all obtained images of the stimulus movement which were of similar size and direction. The mirrors were tilted at the same angle in order to provide against differences in distortion. Sheets of cellophane were fastened over the mirrors by means of rubber bands. The *Ss* closed one eye and traced with a fountain pen the vertical reference line as seen in their mirrors. At the signal "ready" the head was brought into position so that the line drawn on the cellophane coincided with that seen in the mirror. The head was kept in this position until the trial was completed.[8] At the signal "get set" the point of the pen was placed over

FIG. 1

the image of the dot. At the signal "now" the dot started to move, and the *Ss* to trace its course back and forth from top to bottom of the cellophane pages.

(c) Scoring of the test. The *Ss'* responses were scored from a single master key determined empirically by slow-motion records taken on cellophane and transferred to a sheet of millimeter-squared paper. Individual records were placed over this key and the amount of error estimated in terms of the ratio of overshooting or undershooting (in mm.) to the total length of the correct sweeps.

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however, that *E* could duplicate a pattern with sufficient accuracy for our purposes. That part of the slide which carried the pattern was divided into equivalent sectors by vertical lines and *E* checked upon his rate of movement by counting. For example, the first movement to the left was slow, with a count of 1-2 given to each section traversed; the return was more rapid, with a count of only 1 given to each sector.

[8] Movement of the head during a trial introduces a variable reference line and makes scoring impossible. For more accurate work, a biting board or headrest should be supplied.

(d) Reliability of the test. In order to estimate the reliability of our pursuit test, we had 2 groups of 20 *Ss* each repeat the performance on 10 different days, 8 trials per day. Two trials were taken with the *Ss* in one position; then all were shifted to other seats for 2 more trials. This was continued until at the end of the entire experiment every *S* had been seated twice in every possible position in the circle. The average rank-difference correlation between those daily trials in which the *Ss* remained in the same chairs was +0.71 for Group I and +0.64 for Group II. The highest correlation for Group I was +0.89 and for Group II +0.74. The lowest correlation for Group I was +0.50 and for Group II +0.59. Correlations between the performances of the same individuals in different seats averaged +0.75 for Group I and +0.68 for Group II. Correlations between the performances of individuals occupying the same seat at different times was +0.015 for Group I and —0.126 for Group II. This result shows that individual differences are more important than is seat position in determining the score. While we were unable to obtain a Miles pursuitmeter for use in comparative tests, we built a simplified model which we used with individuals in Group I. The correlation of these individual tests with those taken the same day in a group was only +0.46, due possibly to the crudity of our model.

The study of the relative importance of eye and hand dominance in pursuit included the performance of our group test under the following conditions, properly counter-balanced as to practice order: (1) Dominant hand, dominant eye; (2) Dominant hand, non-dominant eye; (3) Non-dominant hand, dominant eye; and (4) Non-dominant hand, non-dominant eye. Tests were given on 2 days, 8 trials each day. Forty college students served as *Ss*; 20 of whom (Group II above) had received considerable practice in the test before the experiment began; the others (Group III) were not familiar with the test. All the *Ss* were right-eyed, as tested by the manoptometer,[6] and all except one were right-handed.

Results. The combined results for all trials are shown, in the table below, in terms of the percentage of error to the total length of the path traversed by the moving dot. The lower the percentage score, the more accurate the pursuit.

TABLE I

SHOWING THE PERCENTAGE OF ERROR MADE BY EACH GROUP AND BY ALL COMBINED

Condition	Percentage of error		
	Group II (practiced)	Group III (unpracticed)	All Combined
Dom. hand with dom. eye	1.63	1.41	1.52
Dom. hand with non-dom. eye	1.60	1.51	1.55
Non-dom. hand with dom. eye	1.59	1.62	1.61
Non-dom. hand with non-dom. eye	1.64	1.72	1.68

From these scores, it is evident that hand dominance plays a much more important rôle in pursuit skills than does eye dominance. On an average there is 0.12% less error for right-handed than for left-handed performance, independent of the eye used; the difference in favor of the dominant eye is only 0.04%, when scores

[6] F. H. Lund, The manoptometer; a new device for measuring eye-dominance, this JOURNAL, 44, 1932, 181.

are averaged independent of the hand used. This is considerably less than the work of Lund would lead us to expect. It may be due either to the type of test used in our experiment or to the fact that we had fewer cases than did Lund. With regard to the reliability of our sample, however, we can report that correlations between two similar trials range from +0.72 to +0.84. That both eye and hand dominance play a more important rôle in the early stages of learning a skill than in later stages is indicated by a comparison of the scores of the practiced and unpracticed groups. This suggests that the phenomena of dominance may be obscured in many tests, due to transfer or cross-education.

VII. Periodicity in Performance

By G. L. Freeman and E. F. Wonderlic

The evidence for periodicity during continuous work is conflicting. Wheeler[1] presents curves of consecutive blindfold maze tracing which show that a pronounced break in performance occurred at regular intervals of approximately 40 trials. Bills[2] found that "blocking" in continuous color naming, was noticeable about 3 times per min. It has been suggested that such fluctuations as these act as automatic safeguards to fatigue and that periodicity is determined either by the refractory periods of the tissues activated or by "some basic metabolic law." Against such assertions may be cited the work of Flugel,[3] who found that periodic breaks in continuous addition were due not to some internal condition, but to the distraction induced each minute by the time signal. Ergographic experiments are frequently cited as showing periodic rhythms, but the authors have found from consultation of published records that more or less regular breaks are just as frequently absent as they are present. In many instances periodicity has been assumed if the most pronounced deviations fall within a rather wide interval of each other. Furthermore, the majority of performances tested have been too complex to provide any crucial evidence. Since in practically all reports the question of periodicity is incidental to some other problem, the authors have proceeded to study the precise relation between deviations in work output and the time-interval between them.

Procedure. The experiment was designed and the apparatus planned so that only one factor in work output would vary at a given time.[4] We used a modified finger ergograph which could be arranged to limit the extent of finger movement if desired. *S* was seated comfortably, blindfolded, and his right arm strapped in the ergograph. A time-mark was made on the record at 5-sec. intervals. In the first series of observations, rate of movement was the variable, the *S* being told to oscillate his finger as rapidly as possible between two stationary stops for a period of 20 min. In the

[1] R. H. Wheeler, *The Science of Psychology*, 1929, 306-307.
[2] A. G. Bills, Blocking; a new principle of mental fatigue, this Journal, 43, 1931, 230-245.
[3] J. C. Flugel, Practice, fatigue and oscillation, *Brit. J. Psychol., Monog Supp.*, 13, 1928, 1-92.
[4] The writers express their appreciation to Miss Elizabeth Phillips and Mr. Clarence G. Browne for assistance in this study.

second series, extent of movement was the variable, *S* oscillating his finger in time with a metronome with the stops now removed. Forty college students served in the experiment. Both series were performed the same day, in counterbalanced practice order, and with a 5-min. rest period intervening. Competition and rivalry between members of a class were the only systematic motivations employed.

Results. Gross inspection of the data gave some slight indication of periodic fluctuations in work output. We then sought to ascertain exactly how rhythmical these fluctuations were and whether the same type of fluctuation would be exhibited by an *S* in both tests. Various techniques of counting the time elapsing between pronounced individual fluctuations were finally discarded as revealing no objective evidence of periodicity. A procedure of dealing with average performance over a small unit of time was then substituted. Records in which extent of movement was controlled and rate was variable were scored by counting the number of finger movements made in every 5-sec. interval of the 20-min. test. Records in which the rate of movement was controlled and extent was variable were scored by averaging the height of the finger movements made during each 5-sec. interval. The 5-sec. values (rate and extent of movement) were averaged for every *S* and the average deviations obtained. Those 5-sec. values which deviated from the average output by one or more A.D. were now regarded as a marked fluctuation, possibly of a periodic nature. The time-intervals between such fluctuations were ascertained, and the frequency of occurrence of each interval accumulated for each of the 40 records. These frequency tables showed that for 21 cases, marked fluctuations in output tended to occur at regular intervals. We further subdivided the cases into those which showed a distribution of intervals about a single mode *(modal rhythm)* and those which showed a distribution of intervals about two modes *(bimodal rhythm)*. The 19 cases which showed no periodicity in work fluctuations were classed as having *multimodal rhythm*. Typical frequency distributions for modal, bimodal, and multimodal rhythms are given below.

TABLE II

SAMPLE FREQUENCY TABLES FOR 3 *S*s OF VARIOUS TIME-INTERVALS OCCURRING BETWEEN MARKED FLUCTUATIONS IN WORK OUTPUT

Modal Rhythm *P*—Part 1		Bimodal Rhythm *L*—Part 1		Multimodal Rhythm *K*—Part 2	
Time interval (in sec.)	Frequency	Time interval (in sec.)	Frequency	Time interval (in sec.)	Frequency
5	1	5	1	5	1
10	2	10	1	10	1
15	5	15	2	15	3
20	7	20	4	20	2
25	5	25	2	25	2
30	4	30	1	30	1
35	2	35	2	35	2
40	1	40	3	40	2
45	0	45	4	45	1
50	1	50	0	50	2
		55	2	55	1
		60	1	60	1
		65	1	65	1

While 21 of our *Ss* showed more or less regular fluctuations in their performance, only 14 preserved a modal or bimodal rhythm in *both* tests; and if the modal rhythm alone is taken as evidence of periodicity, there are only 10 positive cases.

Two conclusions seem possible from this data: either (1) some *Ss* do not exhibit periodicity in performance; or (2) there are factors even in a controlled work situation which act to mask the periodicity effect. Assuming that the second conclusion offers the more useful working hypothesis, the authors have examined their data for the effect of extraneous factors. Motivation and interest may have been lacking in the negative cases, but this is difficult to ascertain. One factor which can be studied is the relationship between the amount of deviation in major fluctuations of work output and the time-interval between such deviations. We asked, "is it a fact that when there is a very marked increment (and subsequent decrement) in performance, there will be a correspondingly longer period of time elapsing before another marked increment in performance is again registered?" To answer this question, we correlated the average extent of marked fluctuations in work output with the average interval between these fluctuations in each individual record. The Spearman rank-difference method yielded slightly positive correlations in both series, being $+0.234 \pm .03$ when only the rate of movement varied and $+0.217 \pm .05$ when only the extent of movement varied. This indicates a slight tendency for the extent of deviation to influence periodicity of performance, a person showing marked increments also showing greater time-intervals between successive increments. The correlations would have been much higher were it not for the fact that many *Ss* showed great variability in the amount of work increment attained at successive peak-production points. A comparison of the variability in the amount of increment (and subsequent decrement) is perhaps the most significant phase of our study. In general, we found that the *Ss showing the greatest variability in work output gave the least evidence of periodicity.* Those *Ss* whose performance indicated modal or bimodal rhythms had an average variability of 1.7 performance-units while those whose performance was multimodal had an average variability of 2.3 performance-units. In other words, the more regular and even is the rate or extent of continuous finger oscillation, the more periodic will be the increments and breaks in performance. Each *S*'s performance may therefore involve a periodic factor, determined perhaps by some property of the tissues activated, but if that *S* attains considerable work increment at one instance (as a result of reënforcement or otherwise) the interval between that increment and the next will be correspondingly lengthened, and the periodic factor will be effectively masked.

Our observations suggest that if there is a periodicity factor in performance, it is an extremely subtle variable and probably not one which plays a major rôle in performance. There would seem to be intrinsic rhythms in gland and muscle action, but these organs adapt themselves easily to external conditions and their action is often modified through reënforcement from other bodily parts.

APPARATUS

THREE NEW MODELS OF THE SYNCHRONOUS MOTOR CHRONOSCOPE WITH ACCESSORIES

By L. W. MAX, New York University

(I) *Chronoscopes. (1) A simplified chronoscope.* The simplified chronoscope,[1] shown in Fig. 1, was designed because of the writer's conviction that the multipolar field of the Dunlap chronoscope,[2] with its 10 magnets, is not essential to its operation, and that the average student of psychology could more readily grasp the principle of the synchronous motor if only two field magnets were involved. Both of these convictions have been confirmed by subsequent laboratory experience with this instrument. In addition to this reduction from 10 to 2 field-magnets, all the parts were made as small as is consistent with durability, both to reduce operating voltages and to minimize inertia effects. The two field windings, A and B, are designed for 4 v. D.C. as compared with the standard 24 v. required for the Dunlap machine; when used on A.C. a simple toy-transformer suffices, whereas the larger chronoscope requires a lamp-bank or rectifier or a more expensive type of transformer.

(a) Principle of operation. The following explanation, in substance, has been found satisfactory in explaining the chronoscope's operation to students. The voltage in a 60 cycle alternating current builds up to its maximum amplitude twice during each cycle, once in a positive and once in a negative direction. When the A.C. is fed to our field windings, the direction of the current is immaterial and the magnets are energised twice during each cycle, yielding 120 magnetic impulses per sec. When the toothed rotor-wheel, R, is set spinning at proper speed, the tooth which is nearest one of the magnet-poles will be strongly attracted to it as the magnet is energised during a particular half-cycle; but by the time the tooth has been drawn to a position directly opposite the pole, the voltage impulse and resulting magnetic attraction will have fallen to a minimum, and the tooth will pass on unimpeded. By this time another magnetic impulse is being provided by the next half-cycle and the next tooth of the wheel is now attracted. This process occurs opposite both poles at the same instant. If the rotor wheel has 12 teeth,[3] it will rotate at the rate of $120 \div 12$, or 10 revolutions per sec. When the dial-hand is attracted to this rotor by the starting magnet, S, it too will rotate at the speed of 10 R.P.S. Since the dial has 100 divisions, each of these divisions has the value of 1 σ.

[1] This apparatus, with the exception of the third chronoscope, was demonstrated and described at the Spring meeting of the New York Branch, A.P.A., April 13, 1931. Cf. *Psychol. Bull.*, 28, 1931, 633. The chronoscopes and stimulus-key are manufactured by the C. H. Stoelting Co., 424 N. Homan Ave., Chicago, Ill.

[2] Knight Dunlap, The Johns Hopkins chronoscope, *J. Exper. Psychol.*, 2, 1917, 249-252; Methods of using balanced-magnet chronoscopes, *Psychobiol.*, 1, 1918, 445-457.

[3] The use of 12 instead of 10 teeth was suggested to the writer by Professor K. M. Dallenbach.

If D.C. is used, it is interrupted by a vibrating tuning-fork to yield the requisite number of impulses per second. Thus, if a 60 cycle fork is used, 60 impulses per second are provided, and the speed of the rotor is 60÷12, or 5 revolutions per sec., each of the hundred dial units now having the value of 1/500 sec. or 2 σ.

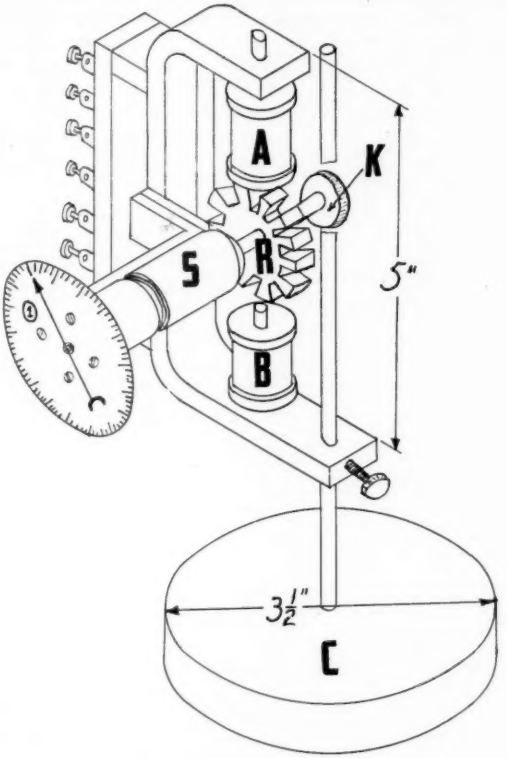

FIG. 1. A SIMPLIFIED CHRONOSCOPE

(b) Clutch mechanism. In the Dunlap clutch the index-hand and its pinion are rigidly fixed to the same shaft as the clutch disk, the whole system moving in and out as a unit. The present design provides instead for a two-piece telescoping shaft; the forward shaft carries the hand and pinion and runs independently on its own bearings; it does not participate in the to and fro clutch movement, which is confined to the rear shaft alone. In consequence the size of the clutch-disk can be reduced and the total result is a considerable lightening of the moving mass and greater precision. To facilitate the task of assembly and accurate adjustment of the parts, the complete

clutch mechanism is designed to be self-centering. To decrease the difficulty commonly experienced by students in starting the synchronous motor chronoscope, one end of our rotor-shaft is extended beyond its bearing and is provided with a knurled knob (K); as a result of this and of the diminished size of all of the parts, the chronoscope starts more easily even on unrectified A.C.

Use as an optic timer. This chronoscope has been so designed as to permit of its use as an optic timer in photographic registration, and it has been thus used by the writer with the string galvanometer. For this purpose, the knurled knob (K) is replaced with the usual episkotister wheels, having either 1, 5, or 10 spokes each (and thus yielding on the photographic film time-lines of 0.2, 0.04, and 0.02 sec., respectively, with the 60 cycle tuning fork). To facilitate the optic adjustments involved in this application of the instrument, the whole chronoscope is adjustably mounted on a small clamp-stand, C, of convenient size.

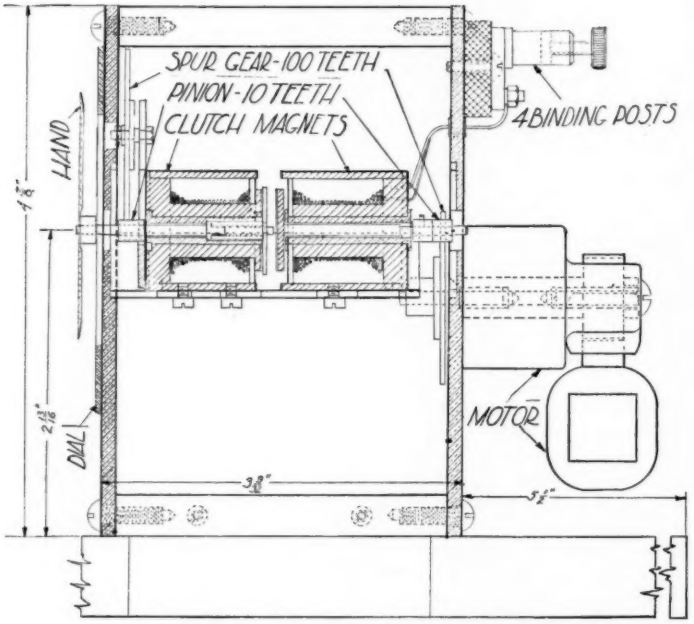

FIG. 2. VERTICAL SECTION OF SELF-STARTING CHRONOSCOPE

(2) A self-starting chronoscope. A second model, a self-starting and self-synchronizing chronoscope, is shown in Fig. 2. It uses a motor of the telechron type, and is plugged directly into the A.C. line without the intermediation of transformers, resistors or rectifiers of any kind. The speed of the shaft is geared up to 10 rotations per sec., which is the operating speed of the clutch and dial-hand. The dial has 100

scale divisions and therefore reads directly in units of 1 σ. The clutch mechanism is the same in principle as that of the simplified chronoscope described above, but the magnets are wound for 6 v., to permit their use with a storage battery. This chronoscope also runs on D.C. with a tuning fork of special design.

The self-synchronizing feature of this chronoscope makes it more convenient for general laboratory and research use. The first model is probably better adapted for purposes of class instruction since its parts are fewer in number and exposed to view, and hence more easily explained to students. Both instruments are small and compact, and easily portable.

(3) Demonstrational chronoscope for large classes. The third model is a larger chronoscope for demonstrational use with large classes. It is also self-starting and self-synchronizing but has a more powerful motor than the usual synchronous-motor unit. It employs an automatic zero-reset device,[4] operable from a distance, and thus frees the demonstrator from the inconvenience of resetting the dial-hand to zero after every measurement. The constituent parts are all of rugged construction, and designed for hard wear. To eliminate inertia-effects and resultant slippage, further modifications were necessary in the clutch mechanism mentioned above. These consisted chiefly of careful milling of the surfaces of the clutch-disk and pole-faces. The clutch-magnets are wound for use directly on a 6 v. storage battery, and are designed to develop an intense magnetic pull. This chronoscope has a dial 12 in. in diam. that can be easily read from the rear of a class-room holding 100 or more students. The opaque-projection method suggested by Ford for rendering the Dunlap dial visible to a large group, is effective but has the serious disadvantage of requiring darkening of the room. The present instrument abandons the customary brass-dial with its low visibility, and substitutes one of white bristolboard with graduations boldly marked in India ink; this is contained in a clock-housing, provided with a hinged glass-door. The complete instrument is enclosed in a dust-proof box which deadens the sound of the motor and of the clutch-impact and prevents student-tampering with the mechanism during the demonstrator's absence.

(II) *Accessories. (1) Neon glow lamp as a visual stimulus.* The neon lamp here referred to is the G.E. type S-2, and is larger than the one described by Miles.[5] Because of its large and flat electrode surface, and its practically instantaneous action, it is admirably adapted for reaction-time work. In some reaction-time investigations,[6] mazda lamps have been employed as visual stimuli, in spite of their failure to reach full incandescence simultaneously with the starting of the chronoscope. The assumption is made that the lag is constant, but this is not always easy to justify, particularly when reaction-times are significantly read to 1-sigma units and the lag of a mazda filament may be as high as 170 σ. The oscillographic method which has been proposed by Kellogg and Davis[7] for measuring the latent time of the mazda filament is itself not free from error. Since the neon lamp under discussion has no filament at all and

[4] Of the type suggested by E. A. Esper, Electrical resetting devices for the Dunlap chronoscope, *J. Gen. Psychol.*, 6, 1932, 478 ff.

[5] W. R. Miles, Neon glow lamps, *J. Gen. Psychol.*, 2, 1929, 545.

[6] For example, V. W. Lemmon, The relation of reaction time to measures of intelligence, memory, and learning, *Arch. Psychol.*, 15, 1927, (no. 94), 11 f.

[7] W. N. Kellogg and R. C. Davis, The measurement of the latent time of electric light bulbs, this JOURNAL, 42, 1930, 300.

is practically free of all lag, an oscillographic check is unnecessary, and this lamp should have wider use in reaction-time work. It is simpler than a pendulum or shutter arrangement and no less accurate. It can be used directly on the D.C. line, the glowing surface of the negative electrode serving as the stimulus. Where D.C. is not available, the lamp can be burned on a 90 v. radio B battery. On A.C., both electrodes will glow and a larger stimulus-surface is afforded, but a time-error is introduced due to the cyclic voltage-variation above discussed; the accuracy still is superior, however, to that of a mazda lamp.[8]

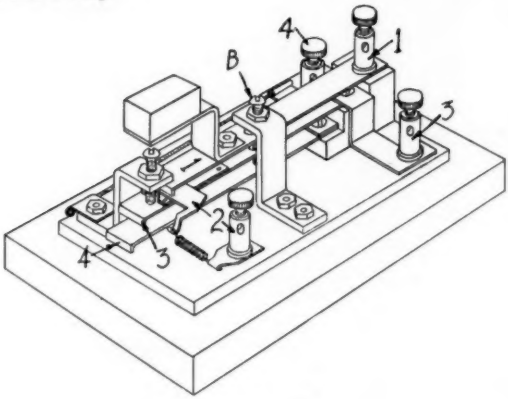

FIG. 3. STIMULUS-KEY

(2) Stimulus-key. The stimulus-key shown in Fig. 3 serves the double purpose of facilitating the use of the high-voltage neon lamp or other stimulus in the chronoscope circuit and of eliminating entirely the necessity of using a relay. It is adapted for use with any of the synchronous motor chronoscopes, or with the Hipp instrument when used without its armature springs. In these instruments, after the stimulus has been applied, it is necessary to reclose the circuit of the stationary or braking magnet. Dunlap's balanced relay has come into use for this purpose. The relay is entirely dispensed with in this stimulus-key by the inclusion of specially arranged blades which are operated by the same downward sweep of the experimenter's hand that applies the stimulus. Blades 1 and 2 close the neon-lamp or stimulus circuit; by this same act, contact between Blade 3 and Binding Post 4 is broken, thus opening the circuit of the braking magnet and starting the chronoscope. As the experimenter's hand continues down, however, Blade 3 comes into contact with Blade 4, thereby reclosing the circuit of the braking magnet and preparing the instrument for the subject's response. The distance between the two nethermost blades (Blades 3 and 4) is adjustable by means of a screw at B, so as to allow a sufficient lapse of time between the application of the stimulus and the reclosure of the circuit (20 σ or more) for the initial action of the clutch. This adjustment is

[8] A smaller neon lamp, the type N1, has recently been developed by the Westinghouse Co., and sells for 15 cents.

easily accomplished. An illustrative reaction-time circuit incorporating this stimulus-key and the simplified chronoscope is shown in Fig. 4.

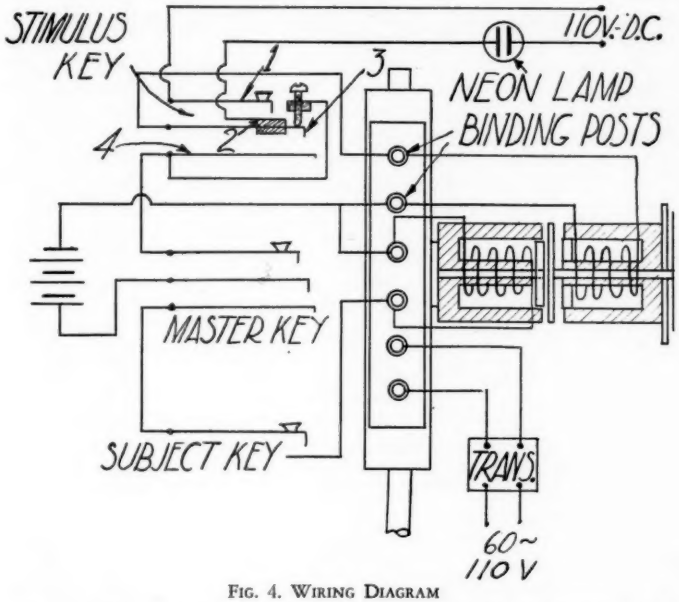

FIG. 4. WIRING DIAGRAM

(3) An inexpensive tuning-fork. Instead of the usual tuning-fork supplied for the synchronous motor chronoscope, the writer uses the Cenco electromagnetic fork (F-6025), rated for 84 d.v. per sec. By adding a pair of slotted weights, the period is easily brought down to 60 d.v. per sec. (or 50, as desired).[9] The graphic stylus attached to one of the prongs is retained, thus making possible a graphic check upon the fork's accuracy and its additional use about the laboratory as a timing device for graphic recording. The total cost is less than half that of the standard chronoscope-fork.

APPARATUS NOTES

THE USE OF GLASS TUBING FOR PULLEYS

The use of pulleys for mechanical remote control of apparatus is beset with certain difficulties. Sometimes the controlling cord must be led along a tortuous path in close quarters; and when the tension on the cord is released it tends to slip off

[9] The writer has on hand several extra weights of the proper dimensions and will be glad to supply them at cost.

the pulley wheel. Furthermore, mechanical pulleys are more or less noisy. These problems will not arise if ordinary pulleys are replaced with short pieces of glass tubing. Suitable lengths of tubing can be heated and bent in any desired direction. The ends of each piece should be fused slightly in a flame so that they will be smooth and will not fray the cord. The tubes can then be inserted in holes in the apparatus, or tied to the framework with ordinary string, and made firm in either of these places with any of the recently commercialized cements. Heavy linen thread or fish-line can be threaded through them with a small steel wire if necessary. Glass tubes, as substitutes for pulleys, are incapable of carrying continuously moving belts at high speeds, but they are dependable, noiseless, and low in friction. Moreover, control cords can be led through them around odd corners where the mounting of a series of ordinary pulleys would be difficult, if not impossible.

Bryn Mawr College. WM. D. TURNER

NOTES AND DISCUSSIONS

Some Neglected Principles in Aptitude-Testing

In this paper I shall present two hypotheses which deserve the consideration of every one who is trying to detect any of the *special aptitudes,* whether actual or potential, that different individuals may possess. Although both hypotheses appear to be defensible, my immediate aim is to exhibit them for consideration, rather than to argue the question of their factual truth. But *if* they are factually true, then all their logical implications are factually true also, and some of these implications are especially important. One of them explains why few tests of special aptitudes have yet been discovered which have much diagnostic value. A second implication is that, except by accident, tests that are constructed according to the present method cannot be much improved—by enlargement, for instance—to the point of utility, for they are fundamentally unsound. Thirdly, the two hypotheses imply a method. If the hypotheses are true, and if the method is feasible, the way seems open to diminish the frequency of misplacements of individuals which the present tests permit. If the method is not feasible, the hypotheses imply that we must either abandon our attempts to diagnose aptitudes and predict successes, or else content ourselves with determinations that are unsatisfactory.

First of all, we must deal with a presupposition that underlies many, and perhaps nearly all, attempts to devise tests of aptitudes, achievements, and skills. That supposition is, that these traits can be *graded,* so that one may somehow determine (directly or indirectly) *how much* of any of them an individual has. Or, it may be conceded that one cannot find an absolute measure of an aptitude, but it has been urged that one can yet measure the deviation of each individual's aptitude from some arbitrary zero, and thereby say *how much more* of it one individual has than another. Thus, one hears such questions as these: As a general, how much better than Napoleon was Lee? As a preacher, how much better than St. Chrysostom was T. DeWitt Talmage? As a composer, how much better than Haydn was Beethoven? As a teacher, how much better than Professor *A* is Professor *B?* Not only are such questions asked, but some psychologists have tried to answer them. All such attempts presuppose that aptitudes, successes, skills, can be graded.

The first hypothesis which we are to consider sets aside this presupposition. It asserts that such a notion as that of aptitude or success is not absolute, but strictly relative to some set of demands which are being made of the individual, and which he is trying to meet; that it has no meaning unless these demands are specified; and that the proper judgment is not to what degree the individual meets these demands, but simply whether he satisfies them or not.

It is admitted that any occupational field proposes more than one set of demands; namely, one set for each specialized 'job' within the field. It is conceded that these sets of demands may be arranged in order, *e.g.* according to the number of contestants who satisfy them, according to the reward that is paid to those who satisfy them, and in still other orders; but the problem of grading the ordered demands presents the same difficulties as the problem of grading aptitudes and successes. We shall return to this question later.

The second hypothesis concerns the manner in which the members of a set

of demands are combined. It asserts that any defining set of demands forms a class, which is the *logical product* of certain other classes, while at least some of the factor-classes which make up the logical product are themselves the *logical sums* of constituent demands. To appraise this assertion we need to recall two definitions.

Logical product. Given two or more constituent classes which we may denote as *A, B, C,* . . . respectively, their logical product is the class $A \cdot B \cdot C$. . . . Each of its members belongs to Class *A,* and also to Class *B,* and also to every one of the other factor-classes. Thus, in one very shallow-minded, haughty and supercilious group, every member is a scholar, *and also* a gentleman, *and also* a judge of good whiskey. This group is the logical product of these three constituent classes.

Logical sum. The logical sum of two or more constituent classes is the class which includes the members of each. For example, if the constituent classes are three, namely, *A, B,* and *C,* their logical sum is the class the members of which belong to Class *A, or* to Class *B, or* to Class *C,* whether they belong to more than one of the constituent classes or not. If *A* is the class of scholars, *B* the class of gentlemen, and *C* the class of judges of good whiskey, their logical sum is the smallest class which includes:

(1) $A \cdot B \cdot C$, *i.e.* the class of individuals who are scholars, and are also gentlemen, and are also judges.
(2) $A \cdot B \cdot \sim C$, *i.e.* the class of individuals who are scholars, and are also gentlemen, but are not judges.
(3) $\sim A \cdot B \cdot C$, *i.e.* the class of individuals who are not scholars, but are gentlemen, and are also judges.
(4) $A \cdot \sim B \cdot C$, *i.e.* the class of individuals who are scholars, but are not gentlemen, and are judges.
(5) $A \cdot \sim B \cdot \sim C$, *i.e.* the class of individuals who are scholars, but are not gentlemen, and are not judges.
(6) $\sim A \cdot B \cdot \sim C$, *i.e.* the class of individuals who are not scholars, but are gentlemen, though they are not judges.
(7) $\sim A \cdot \sim B \cdot C$, *i.e.* the class of individuals who are not scholars, and are not gentlemen, but are judges.

The relationship between the logical sum and the logical product of three constituent classes is imperfectly shown in Fig. 1.

Note that each of the seven subclasses which additively constitute the logical sum (*i.e. A,* or *B,* or *C*) is itself a logical product. Note also that the logical sum (*A,* or *B,* or *C*) excludes no individuals except those included in the logical product $\sim A \cdot \sim B \cdot \sim C$: in this example, those who are not scholars and are not gentlemen, and are not judges.

Note that the requirements of admission to any class, such as the class of scholars, Class *A,* may be met in a variety of ways. It is essential that the individual should be master of the details and internal principles of *some* one or more particular fields, and relate his special information to a wider field of human interest, knowledge, and problems. If he satisfies this condition, it makes little difference in which field he is expert. Hence, the class of scholars includes those who have this mastery of archeology or of Greek, or of mathematics, or of logic, or of physics, or of psychology, or of engineering, or of painting, or of sculpture, or of history, or of literature, or of what you may choose otherwise. In other words, the class of scholars is the logical sum of the cultured experts in any one or more than one of these particular fields.

Respecting Class *B*, which in this instance is the class of gentlemen, it also appears to be the logical sum of certain constituent classes. Perhaps the essential requirement is that the individual should habitually manifest magnanimity, understanding, generosity, and consideration for the rights and feelings of others, and express these attitudinal patterns by responses that are so interpreted by the group to which he is attached. Thus, we find a difference in the external behavior-pattern of the British gentleman, the Chinese gentleman, the Turkish gentleman, the Abyssinian gentleman: a particular individual may gain access to the class of gentleman if only he manifests the social behavior-pattern of *any* of these (and the other) subclasses, provided that his intention is evident and understood.

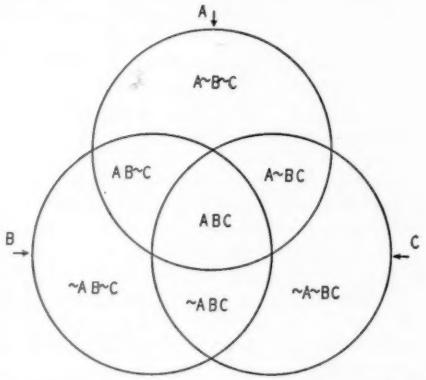

FIG. 1. A DIAGRAM SHOWING THE RELATIONSHIP BETWEEN THE LOGICAL SUM AND THE LOGICAL PRODUCT OF THREE CLASSES, *A*, *B*, AND *C*, REPRESENTED BY THE THREE CIRCLES

The symbols $\sim A$, $\sim B$, $\sim C$ denote the classes the members of which are excluded from *A*, *B*, and *C*, respectively.

Similarly, with respect to Class *C*, which includes all judges of good whiskey. The essential requirement is that the judge should select the good from the collection of good and bad, so as to please his friends with his offerings, and conventionally appraise the judicious selections which they may offer him. It is indifferent whether he judges the whiskey by pouring it into a glass and sniffing it, by rubbing it between his fingers, by rolling it over his tongue, by swallowing it and then exhaling, or by applying some quick and certain chemical test. Hence the class of "judges" includes all who discriminate, by one method or by some other, but who use effectively the means that they do employ.

We may now abandon this particular illustration, and turn to the general assertion which is formulated in the hypothesis; namely, that the class of individuals who succeed in any particular job includes all those and only those individuals who meet the classes of demands, *A*, *B*, *C*, etc., all at once. In other words, the successful individuals have command, all at once, of certain *classes* of skills; these classes enter as logical factors into the production of occupational aptitude or success; but since,

by hypothesis, any factor-class may be a logical sum of particular skills or classes of skills, our judgment takes the following form.

Given a specified occupation, if an individual is successful, apt, or skilled in it, he is a successful *A*-individual, *and also* a successful *B*-individual, *and also* a successful *C*-individual, and so through the list of factor-classes of skilled individuals. But, he is a successful *A*-individual if he has command of the special skill a_1, *or* of the special skill a_2, *or* of the special skill a_3, *or* any other special skills of which the Class *A* is the logical sum. Likewise, he is a successful *B*-individual if he has command of the special skill b_1, or of the special skill b_2, or of the special skill b_3, etc. Similarly, he is a successful *C*-individual if he has command of *any* of the special skills c_1, c_2, c_3, etc. So on through the list of factor-classes.

Hence, he succeeds in this occupation if he satisfies the occupational demands a_1, or a_2, or a_3, . . . or a_n, *and also* b_1, or b_2, . . . or b_n, *and also* c_1, or c_2, . . . or c_n, *and also* by any means whatever the rest of the factor-classes of demands.

Note that he is an occupational failure if he fails *all* the constituent demands in *any* factor-class such as *A*, or *B*, or *C*, etc.; whereas he is an occupational success if he satisfies *at least one* demand in *each* factor-class.

Thus, according to the two hypotheses, the diagnosis of vocational aptitude or success depends upon the simultaneous *combination* of several *classes* of skills, some members of each class being *interchangeable* with other members.

The conventional manner of making aptitude-tests, however, takes no account of combination or of interchangeability. The tester tries to detect as many of the special skills, such as we have denoted by the small letters, as he can; he then assigns a weight to each, and takes the sum of the weights for the subject's absolute score in the test.

I shall now mention four special cases, which go to show the kind of result which this procedure can yield.

Case 1. The subject has command of at least one skill in each of the factor-classes *A, B, C,* . . . , and of many skills in at least some of these classes. His score in the test will be high; the correct diagnosis is 'success.'

Case 2. The subject has command of only one skill in each of the factor-classes. Unless there is great disparity in weighting, his score will be low; but the correct diagnosis is 'success.'

Case 3. The subject has command of none of the skills in at least one of the factor-classes, and of only a few skills in any of the others. His score will be low; the correct diagnosis is 'failure.'

Case 4. The subject has command of none of the skills in one of the factor-classes, but has command of many of the skills in each of the others. His score will be high; the correct diagnosis is 'failure.'

From these considerations it is evident that a high score may be associated with either success or failure, and a low score with either failure or success.

For example, consider the special skills that one must have to be an operatic tenor-singer. I have some of them. I am sensitive to small differences in pitch; I have good breath-control; I can sing what I do sing in key; I can enunciate distinctly; I can use good phrasing. Now if you weight each of these skills and take the sum of their weights, you will have to give me a finite score in aptitude as an operatic tenor-singer. But I fail one requirement that every operatic tenor must fulfil, in that I cannot make a musical sound vocally on a high note. The other special tenor-skills which I have therefore contribute nothing to my aptitude for that vocation, nor do the other tenor-skills which I lack contribute further to my ineptitude as a tenor.

This example has a perfect analogy in the field of differential diagnosis in medicine. Consider some disease, whether actual or hypothetical, which can be unequivocally determined only in its later stages, or in a *post-mortem*. Meanwhile, the physician bases his judgment on which he calls a "syndrome," which is a class of *concurrent* symptoms, or concurrent classes of symptoms. Thus, the syndrome also is a logical product, of which some, at least, of the factors may be logical sums of particular symptoms, each of which has the same diagnostic significance. Let us suppose such a syndrome, formed by the concurrence of three classes of symptoms; namely *A, B,* and *C,* each of which is a logical sum of particular symptoms. Then, we may readily imagine three patients who have equivalent syndromes. The first patient has the special symptoms a_1, b_1, and c_1; the second patient has the special symptoms a_1, b_2, and c_1; the third patient has the special symptoms a_2, b_1, and c_2. Thus the first patient and the second patient have two symptoms in common; namely, symptoms a_1 and c_1. The first patient and the third patient have one symptom in common; namely, symptom b_1. The second patient and the third patient have no special symptoms in common. Nevertheless, by hypothesis, the three patients are correctly judged to have the same disease. I have been told that instances of this type occur frequently in medical practice.

Note that if the patient has *at least one* special symptom in *each* of the factor-classes of symptoms in the syndrome, the diagnosis is positive; if he lacks *all* the symptoms in *any* of the factor-classes, the syndrome is incomplete, and the diagnosis must be negative. In this event, the physician may do some foolish things, but he probably will not say, for example, that the patient has two of the three of the classes of symptoms that concur in the syndrome, and that therefore he has 66.6% of the disease. But if he should do this, he would be following the same general procedure as the aptitude-tester employs in making a point-scale rating.

I believe that the hypotheses which we have considered are factually well based. I may be wrong, but unless I am wrong, we cannot hope to do much with aptitude-testing *in the placement of particular individuals* until we replace the point-scale system of rating by one that is radically different. By enlarging our lists of questions, and by differential weighting, we can still improve our predictions a little, but we do not thus remove the defects that inhere in the point-scale method. If we should thus improve our diagnoses, the reason will be that we have successfully groped into a part of the procedure which our hypotheses indicate as being necessary. This procedure now calls for detailed consideration.

To begin with, we set aside the presupposition that an individual's occupational skill, ability, aptitude, or success can be graded. To say that he has a certain quantity of any of these traits is like saying that a patient has a certain quantity of a disease. Next, recognizing that the judgment of aptitude is relative to a class of concurrent demands, we undertake to set up, first, a complete statement of these factor-classes of demands, and secondly, a complete statement of the different ways in which each class of demands can be met. Then, if the individual, in any way whatsoever, can meet each of these classes of demands, all at once, we judge him to be apt in the occupation that is thus defined; otherwise we judge him to be inept.

For example, we may consider the various particular jobs within the field of truck-driving. If the job is that of driving the truck from one place to another in a freight-yard, through short distances, and with long pauses for loading and unloading between trips, all individuals can satisfy it who can handle the truck, and avoid

accidents. Many individuals can satisfy these two concurrent demands. If, however, the job requires long trips, which have to be made within some specified time, it makes the additional demand of speed. Some individuals who meet the first two demands will fail this one. Suppose we add the demand that the driver should cover some specified mileage without wearing out his brakes. Those individuals will fail it who do not look ahead, and detect portents of changes in the traffic and react to them, instead of running into traffic situations which require sudden and powerful braking. If the job requires that the driver make personal delivery of his freight, as the driver of a milk-delivery truck must do, some demands of social adaptability have to be added. If he has also to make collections and handle money, still another set of demands are imposed.

Note that if the job is the one which we last mentioned, we may apply the test of ability to meet each class of demands in succession. Those individuals who fail the first class, in some manner or another, are definitely excluded. Those who meet the first set, but fail the second, are next excluded. Each factor-class of demands acts as a sieve, which excludes some of the unfit, and allows the fit, and also some unfit, to pass. Thus, a perfect test would apply all the factor-classes of demands that the job makes,, and would exclude all individuals who failed *any* factor-demand by *all* the interchangeable means that may exist. This implies a "job-analysis" that is thorough, and which is also different in kind from the types with which we are most familiar.

This procedure is conceivable, is it also feasible? I do not know. I doubt whether any person could thus determine and classify the requirements of very many jobs during his professional life-time. But if our premises are correct, nothing short of this procedure will suffice. The case reminds one of the requirement: *Draw a map of the other side of the moon.* As every reader knows, one half of the moon's surface is never turned toward the earth, and for this reason it can never be observed from the earth. To make a true map, one has to locate an observer, or some recording instrument such as a camera, out in the space beyond the moon, and make the observations from there. I do not expect this to be done soon, but I know that until it shall have been done, no verifiable map of the other side of the moon will be made. Similarly, our hypotheses imply that *if* we should ever devise any satisfactory tests of special aptitudes, those tests will utilize the pattern of procedure which we have just considered. If the conditions which it stipulates cannot be fulfilled, it is too bad for our aspirations.

But, if we cannot formulate all the classes of demands which a particular job imposes concurrently, can we still reduce the number of false diagnoses? Indeed, we can, if we can satisfy one condition. That is, that for each factor-class that we do consider, we list all the interchangeable skills that satisfy it. Then, beginning with the first factor-class, we test the subject by each of its interchangeable requirements in order, until he *meets one* or else *fails all.* If he fails all, he is instantly disqualified; if he meets one, he is eligible to test by the next factor-class. So we proceed, until he *fails one* of the factor-classes of demands, or else until he somehow has *met all.* Thus, each factor-class of demands will eliminate some failures, but no successes; and it is conceivable that at least in some jobs, we need only to apply exhaustively a few well chosen factor-classes of demands in order to eliminate nearly all the inept applicants.

Did space permit, I could mention some indications that at least some practical testers seem to be groping toward this form of procedure. But it seemed to me to be

possible that the goal may be more quickly achieved, or else proved to be impracticable, if the seemingly necessary procedure is made explicit.

Certainly, the demand which is now being voiced in the present economic emergency, that some means should be found of finding the jobs which particular individuals are capable of doing, calls for a close examination of fundamental principles, rather than rushing headlong into a feverish and chaotic program of testing, such as we engaged in during the World War.

American University H. M. JOHNSON

AMBIGUITY OF DESCRIPTIVE TERMS FOR FEELING AND EMOTION

Much confusion has arisen in the field of feeling and emotion due to the experimentalist's uncritical assumption that the common terms covering *feeling* are sufficiently standardized in meaning to be used as categories of report without further definition. Unfortunately the same term may be used to refer to widely varying experiences.[1] It may have either a sensory, a relational, or an objective meaning, depending upon what aspect of the experience is stressed. The *sensory* type signifies that sensory material is primary in the *O*'s experience. The *relational* type is of a logical nature and refers to the environmental situation, or the relation of a complex group of stimuli.[2] The *objective* type, common in hedonic judgments, refers to the feeling as an attribute of some object, and often merely signifies the presence of the object in consciousness. If we extend *feeling* to include the aesthetic experience, we find a fourth, or *subjective* type, refering to an immediate, subjective, personal experience, not sensory in nature, and resembling the philosopher's immediate feelings of value or the intuitive appreciation of beauty.

In order to test the distribution of these common meanings, definitions of *fear, pleasantness* and *unpleasantness, aesthetic feeling,* and *beauty* were obtained from a large number of women undergraduates in an introductory course in psychology. The number of definitions in the four instances ranged from 79 to 131. The students were instructed to "tell in your own words how you use the term—what it means to you."

The following excerpts illustrate the descriptions obtained for each word and also the categories of classification in which these descriptions can be placed:

FEAR. *Sensory:* "An awful feeling in my stomach, and cold, clammy hands;" "A sort of contraction in the throat, and a tight feeling in the diaphragm." *Relational:* "Something is present which I would like to avoid;" "Being aroused or startled by something which may cause either a short or a long period of anxiety." *Objective:* "Something we are afraid of either mentally or physically;" "Something to draw away from, to escape from."

PLEASANTNESS and UNPLEASANTNESS. *Sensory:* "A light, airy feeling;" "A certain buoyant, light, happy sensation;" "Repulsive, disgusted feeling usually connected with some nausea;" "Tense feeling." *Relational:* "Freedom from conflict;" "A situation in which the individual is in rapport with the stimulus;" "When nothing is right;" "Incongruity in a situaton." *Objective:* "Anything that appeals to my senses;" "Something I like;" "Something I dislike;" "Applies to those things that have unhappiness connected with them."

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[1] W. A. Hunt, The meaning of pleasantness and unpleasantness, this JOURNAL, 45, 1933, 345-348.

[2] H. Cantril and W. A. Hunt, Emotional effects produced by the injection of adrenalin, this JOURNAL, 44, 1932, 300-307.

AESTHETIC FEELING. *Sensory:* "An airy feeling;" "Visceral experiences." *Relational:* "When all the elements in experience balance;" "When you see a beautiful scene of nature or hear music." *Objective:* "Something which appeals favorably to my whole senses;" "Something which shows good choice." *Subjective:* "A feeling of artistic value;" "An inner feeling of beauty."

BEAUTY. *Sensory:* "A sense of ease and enjoyment;" "A feeling of relaxation and pleasantness." *Relational:* "Perfection in a situation;" "When something fits." *Objective:* "Something with good lines;" "Something perfect." *Subjective:* "An inner feeling;" "An immediate appreciation of artistry."

The frequency with which these categories are employed is shown in the following table:

	Sensory	Relational	Objective	Subjective
Fear	80	42	5	0
Pleasantness and Unpleasantness	22	37	20	0
Aesthetic Feeling	25	46	13	38
Beauty	2	4	121	4

Too often in working with feeling and emotion we have assumed that the use of a common term of report guarantees the presence of the same experience in consciousness. This is not so, and until we correct our experimental procedures by carefully defining such categories of report we can expect much confusion.

Connecticut College for Women WILLIAM A. HUNT

THE FORTY-SECOND ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

The forty-second annual meeting of the American Psychological Association was held at Columbia University, New York, N.Y., on Wednesday, Thursday, Friday, and Saturday, September 5, 6, 7, 8, 1934. A new record in registered attendance was established, the total being 847 persons, classified as follows: 177 Members, 375 Associates and newly-elected Associates, and 295 persons not affiliated with the Association.

At the business meeting, 3 Members were elected, 27 Associates were transferred to the status of Member, and 131 Associate Members were elected. On recommendation of the Council of Directors it was voted to decide the question of accepting the invitation for the 1935 meeting from the Department of Psychology of the University of Colorado at Boulder, Colorado, or of accepting the invitation for the 1935 meeting from the Department of Psychology at the University of Michigan at Ann Arbor, Michigan, by means of a mail ballot of the entire membership of the Association.[1] Newly-elected officers are: President, A. T. Poffenberger; Secretary, Donald G. Paterson; Directors, John E. Anderson and Edward S. Robinson; Division of Anthropology and Psychology, N.R.C., Clark L. Hull, Donald G. Paterson, and Robert S. Woodworth; S.S.R.C., A. T. Poffenberger; A.A.A.S., Walter R. Miles and Christian A. Ruckmick. William T. Heron, Hulsey Cason, and the Secretary were elected as

[1] The poll closed in Minneapolis on October 15, 1934 and the tabulated vote was as follows: 404 to accept the invitation from the University of Colorado, 572 to accept the invitation from the University of Michigan. In accordance with the results of this mail ballot, the Forty-Third Annual Meeting will be held at the University of Michigan. The dates of the meeting will be September 4, 5, 6, 7, 1935.

the Program Committee for 1935. Elmer K. Culler was elected as a member of the Committee on Precautions in Animal Experimentation.

The Secretary announced the confirmation by the Council of Directors of the appointment by the Board of Editors of Herbert S. Langfeld to be editor of the Psychological Review, the resignation of H. S. Langfeld as editor of the Psychological Monographs, the appointment of Joseph Peterson to be editor of the Psychological Monographs, the resignation of E. S. Robinson as editor of the Psychological Bulletin, and the appointment of John A. McGeoch to be editor of the Psychological Bulletin.

In view of the fact that the relationships between the American Psychological Association and the official publications of the Association have never been explicitly set forth in the By-laws of the Association, it was voted to authorize the President to appoint a committee consisting of three Members and the President and the Secretary as *ex-officio* members to study the relationships involved and to formulate suitable amendments to the By-laws to be presented at the September, 1935 meeting of the Association. In accordance with this action, President Poffenberger later appointed John E. Anderson, Harvey A. Carr, and Samuel W. Fernberger to membership on this committee with John E. Anderson as Chairman. The report of the Program Committee was approved including the recommendation that four-day meetings be continued and that no author shall submit an abstract on more than one topic. The Association voted to discontinue the Committee on Research in the Social Sciences and also to discharge the Committee on the Ph.D. Degree in Psychology. Affiliation with the Inter-Society Color Council was continued with Forrest Lee Dimmick, Clarence H. Graham, and Sidney M. Newhall as delegates. The Committee on Motion Pictures and Sound Recording Devices in Instruction of Psychology was continued with Walter R. Miles as Chairman and the following as members: Edgar A. Doll, Paul S. Achilles, Leonard Carmichael, Willard Valentine, and Milton Metfessel. The proposal of this Committee to arrange for the publication in Psychological Abstracts of brief descriptions of available films was approved. A new Committee on the Experimental Study of Suggestion was appointed with Herbert Woodrow as Chairman and the following as members: Clark L. Hull, Olga Bridgman, A. T. Poffenberger, and Paul C. Young. Certain changes in the By-laws were authorized whereby the preferential system of voting will be followed in all elections conducted by the Association. A proposal to hold the annual meeting in October was voted down but the question of a late December meeting versus an early September meeting was reopened by authorizing a mail ballot on this question, the results of which are to be used for the guidance of the Council of Directors in presenting recommendations at the 1935 meeting.[2]

Twenty-two sessions for formal papers were scheduled and 129 papers were presented. Papers on animal psychology predominated, five sessions being devoted to reports under this rubric. Active research interest in physiological psychology is also attested by the large number of papers in this field. An actual demonstration of conditioning upon the basis of faradization of the cerebral cortex of a dog was conducted by Dr. Roger Brown Loucks of Phipps Clinic. This innovation met with enthusiastic approval. Vigorous research activity is also apparent in the following topics: sensation and perception, memory and learning, abnormal psychology, galvano-

[2] The mail ballot on this question resulted as follows: 321 in favor of late December meetings, 653 in favor of early September meetings.

metric methodology, mental testing, personality measurement, child development and student personnel work.

On Thursday afternoon six instructional films were exhibited and on Thursday evening nine research films were shown. It is apparent that rapid strides are being made in film making—amateurish films are tending to disappear. Especial mention should be made of the sound film exhibited by Dr. Arnold Gesell because it is the high point achieved so far in the making of psychological films. The Round Table on Instructional Films, with Walter R. Miles as Chairman, was well attended, a fact indicative of the growing recognition of the importance of the moving picture as an instructional device. One of the twenty-two sessions for formal papers was devoted to papers on Clinical Psychology and a Round Table on Clinical Psychology was also scheduled with Dr. Edgar A. Doll as Chairman. Stress was placed upon the need for a broad conception of clinical psychology so that clinically trained psychologists might find more adequate employment outlets in the educational and vocational guidance movement at the adult level as well as at the pre-adult level. The Section on Clinical Psychology elected Dr. Clara Harrison Town as Chairman for 1934-1935, elected Dr. Edward B. Greene as Secretary for 1934-1937, and elected Dr. Hyman Meltzer to be a member of the Executive Committee for 1934-1937. A total of 5 new Members and Associates were admitted to this section. The Round Table on the Use of Electric Shock in Psychological Experimentation under the chairmanship of Dr. G. R. Wendt was devoted to reports prepared by members who attended the same Round Table in 1933. Plans were formulated for publishing these reports in the March, 1935 issue of the Psychological Bulletin.

On Friday evening, the Presidential Address, "Aspects of Learning," was given by Joseph Peterson. In brief, President Peterson examined the results of a number of experiments, ranging from investigations of learning to react to upright images on the retina and to reversals of auditory receptors, through cutaneous localizations, to several more specific, unsolved problems relating to maze learning by white rats. Various hypotheses were considered and the need for crucial experiments in the several fields to test these hypotheses was emphasized as essential to scientific progress. In the fields of learning covered in the Address, the rôle of consistency factors was stressed as against such factors as frequency, recency, affective tone, and specific "satisfiers" in the selection and fixation of successful responses.

Following the Presidential Address, the University entertained members in the Men's Faculty Club.

Dr. Poffenberger, his colleagues, and the administrative officers of Columbia University have earned the gratitude of all psychologists who attended the forty-second annual meeting because they did everything humanly possible to make the meeting a memorable success.

University of Minnesota DONALD G. PATERSON

James Mark Baldwin: 1861-1934

The death of James Mark Baldwin in Paris on November 8, 1934, at the age of 74 years, recalls to the older generation of psychologists a brilliant early career which, so far as psychology is concerned, had an early end. He was born in Columbia, South Carolina, in 1861, took his A.B. at Princeton in 1884, and spent the next year at Leipzig and Berlin. In 1886 he was recalled to Princeton as an instructor in modern

languages, and he received the master's degree there in 1887. For the next three years at Lake Forest University and from 1890 to 1893 at Toronto he was professor of psychology. His doctor's degree was conferred in 1889 by Princeton, which called him to the chair of psychology in 1893. His last teaching in the United States was at Johns Hopkins from 1903 to 1909. The next 5 years were spent in Mexico, assisting in organization problems at the University. His life since that period has been passed in France, where from 1919 to 1924 he was professor at *L'école des hautes études sociales*, Paris.

The American period was prolific in publications, beginning with textbooks, *A Handbook of Psychology*, 1890, second edition 1891; *Elements of Psychology*, 1893. His high reputation was established for the time by his *Mental Development in the Child and the Race*, 1895, and *Social and Ethical Interpretations of Mental Development*, 1897. *The Dictionary of Philosophy and Psychology* appeared in three volumes from 1901 to 1905; a volume of collected essays and a short discussion of *Development and Evolution* in 1902. Next came, in three volumes from 1906 to 1911 a treatise on genetic logic entitled *Thought and Things*. An essay on *Darwin and the Humanities* appeared in 1909; *The Individual and Society* in 1911, and in 1915 the culmination of his genetic logic in *Genetic Theory of Reality*.

This closes the period of his scientific and philosophical writings. Honors came to him early; for example, he was vice-president of the International Congress of Psychology in London at the age of thirty-one, and president of the American Psychological Association at thirty-six. With the honors bestowed upon him by Allied governments in recognition of his writings for the Allied cause during the World War we are not here concerned.

In the last edition of *American Men of Science* his subjects are stated as logic and esthetics, psychology not being mentioned. Although he was one of the earliest psychologists of modern times to recognize the fundamental influence of motor processes, and perhaps the pioneer psychologist to make imitation a basic concept, his theories on child psychology suffered from a lack of factual basis. His *Mental Development* was appropriately dedicated to his two little daughters, for the only actual observations recorded were made on "H. and E." under completely unstandardized conditions. This was intentional on his part. What made his observations so valuable in his opinion was the fact that he made them under the influence of his theories! "Only the psychologist can observe the child, and he must be so saturated with his information and his theories that the conduct of the child becomes instinct with meaning for his theories of mind and body," he wrote.[1] Professor T. L. Bolton, reviewing the book in 1895, commented on this statement: "This has been Professor Baldwin's great mistake, and it has rendered his book in some parts a barren waste of speculation."[2] It accounts for the fact that the name of this brilliant thinker seldom if ever appears in present-day volumes on child psychology.

Vassar College MARGARET FLOY WASHBURN

[1] *Mental Development in the Child and the Race*, 1895, 37.
[2] This JOURNAL, 7, 1895, 144.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

A Hundred Years of Psychology: 1833-1933. By J. C. FLUGEL. New York, Macmillan Co., 1933. Pp. 384.

Professor Flugel has added to the recent histories of scientific psychology a very readable, interesting and informative book. He orients the psychology of the last one hundred years with respect to the psychology of the present; and, if his view of the present seems to an American a little like the present as it appears in London, still one does not expect or want an author entirely to suppress himself even in an historical text.

The book is divided chronologically into four parts. Part I (62 pp.) deals with the status quo in 1833. It is the least interesting part of the book, and one cannot avoid the conviction that Flugel is not interested in the psychology of 1833 except as something to get away from. Constantly he is looking forward. Thus he has seven pages devoted to Herbart, but two and a half of them consider Herbart in relation to Freud in a way that makes Freud seem interesting and Herbart dull. He has a ten-page chapter ostensibly on phrenology, but less than half the chapter deals with the history of phrenology about which there is so much to say that is interesting; the rest considers the problem of faculties in phrenology and in the mental tests, with a suggestion to the reader that he note the great advance from Spurzheim to Spearman. The result of this method of constant allusion to the present from the past leaves one for a time in doubt about the audience to whom the book is addressed: the reader of the first two parts of the book should be very sophisticated about the present (or else he will be bewildered) and very ignorant about the past (or else he will be bored).

Part I attempts to give a picture of psychology as it might have been realized in 1833 by a hypothetical student with such divine guidance that he would be able to select those topics that are still to be important to psychologists in 1933. Flugel himself notes the artificiality of this picture, but he might have made it a little more plausible. For instance, he has this student of 1833 knowing the facts of stereoscopy (date cited thrice: pp. 12, 28, 62), presumably because Wheatstone's colleague, Mayo, had mentioned the stereoscope in his *Human Physiology* in 1833, although Wheatstone did not write his own description until 1838, and although Johannes Müller did not know about the stereoscope before 1838. That is asking the student to be wiser than Müller, who certainly had abundant interest in these matters. The picture of how the 1833-equivalent of the modern psychologist felt about his field of scientific interest has, I think, yet to be painted—for I, too, once attempted to make this picture with no greater success. The prejudices and the ignorances of 1833 are just as important as the knowledges.

Part II (40 pp.) takes up the first period of the hundred years: 1833-1860. It is short but adequate. It deals with James Mill, Bain and Lotze, with Johannes Müller, Weber, and Helmholtz's measurement of the nerve-rate, and with Elliotson, Esdaile and Braid on hypnotism. The author has not yet struck his stride which he reaches only in the last two-thirds of the book.

Part III (114 pp.) covers the period 1860-1900. It is impracticable to mention

all the topics and names: evolution (Darwin and Spencer), animal psychology (ten men from Spalding to Hobhouse), individual psychology (Galton), child psychology (Preyer, Hall), race psychology (Baldwin, Tylor), social psychology (Spencer, Tarde), religious psychology (Hall, Starbuck), systematic textbooks (Brentano, Ward, James, and nearly a dozen other lesser ones), psychophysics (Fechner and G. E. Müller), sensation (a chapter to Helmholtz, and very brief mention of Von Kries, Hering, Mach, Von Frey, Zwaardemaker, Stumpf and a few others), the founding of experimental psychology (Wundt), its expansion (Ebbinghaus, G. E. Müller, Hall, Cattell, and half a dozen others), physiological psychology (Broca, Fritsch and Hitzig, Cajal), French psychology (only 3 pp.). Flugel is interested here and he writes well. One notes the British atmosphere in his evaluation of topics; yet he graciously acknowledges an indebtedness to Gardiner Murphy's *Historical Introduction to Modern Psychology* and to my *History of Experimental Psychology,* and it may be that the availability of these books has been one of his aids in approaching the international catholicity which he sets forth as an ideal in his final chapter.

Part IV (138 pp.), describing the present of 1900-1933, is where Flugel is most at home. The chapters on the Würzburg school, McDougall, and Freud are very clear, lucid and happy expositions of topics where the author knows his subject thoroughly and presents it sympathetically to the reader. Here the American reader will sense the British atmosphere more, in the space and favorable attention given to McDougall and Spearman as contrasted with the inadequacy of the treatment of Gestalt psychology and behaviorism. Perhaps Flugel for this period lacked the sources to draw upon, or perhaps his enthusiasm ran away with him, as it must have done when he gave fifteen pages to Freud and then lumped Adler, Jung, Kretschmer and Jaensch into ten pages. Thus the lacunae become very prominent in this part. For instance, Gestalt psychology gets an entire chapter of eleven pages, but less than half is allotted to what is only a superficial history of the movement. The rest is criticisms of the movement and comments about it, with more than two pages given over to a positive account of Spearman's views, as if a vision of Spearmanic truth might make the evaluation of Gestalt psychology easy.

There can be no doubt that Spearman is Flugel's fondest prepossession. Spearman and his factor school are given sixteen pages. Mental tests in general get only seven pages (and Spearman has most of one of these seven). Thorndike is not mentioned in these chapters. Terman's and Kelley's names merely occur in lists. It is a startling disproportion, enhanced by such phrases as the following: "It is to Spearman, far more than to anyone else, that we owe our present knowledge as to the structure or 'make-up' of the human mind. . . . His achievement . . . is one of the most remarkable in the whole history of psychology."

The greatest deficiency, however, appears in respect to the topic of sensation. There is a six-page chapter, which, beginning with Head, devotes four pages to somesthesia (and more than a page to myself!) and mentions Henning and the translation of Helmholtz's *Optik* into English. There is not a sentence in this book about the contributions of the twentieth century to the psychology of vision and of hearing. There is no mention of Troland, Ferree, Johnson, Cobb and the others, or of the Optical Society of America and the psychological researches published in its journal. There is no mention of the way in which audio-oscillators (valves) have transformed research on hearing, or of the researches of Fletcher and his as-

sociates at the Bell Telephone Laboratories, or of the Wever-Bray effect and the consequent experiments of Adrian, Davis, Forbes, Crowe and others. There is nothing about tonal localization, and nothing about the constancy phenomena in perception. This is not the author's field, but he could have got up the content from Troland's *Psychophysiology* or Murchison's *Foundations of Experimental Psychology.*

Flugel presents 1900-1933 as the period of the schools and he gives such an insightful list of popular controversial principles of systematic orientation for the entire century, that I wish to advertise it by repeating it here. Here are the paired opposing principles that have been most important in the three periods:

1833-1860: mechanism *vs.* activity; and association *vs.* faculties.

1860-1900: system *vs.* experiment; content *vs.* act; and general *vs.* differential.

1900-1933: structure *vs.* function; elements *vs.* Gestalt; introspection *vs.* behavior; mechanics *vs.* purpose; and conscious *vs.* unconscious.

The above makes it look as if schools were multiplying, but I do not think that they are. There is more psychology and there are more ideas and therefore more systematic dimensions in which a choice may be made. The phenomenon of the multiplication of systematic issues results from richness of thought and the enlarged scope of psychology. The schools, on the other hand, depend upon personal groups and cleavages, and thrive nowadays—or so I had thought—only in Germany. We had schools in America from 1890 to 1910 when Wundt's students ruled the laboratories with German polemics. Now we have discussion, and behaviorism is not a school, but extends all the way from Watson to Tolman (whom Flugel does not mention), a range too broad to be defended on any set of common values.

It is thus that Flugel accuses America of accepting behaviorism uncritically (p. 264). He cites two American newspapers which said of Watson's *Behaviorism* in 1924 that "it marks an epoch in the intellectual history of man," and that it is "perhaps the most important book ever written." It may be that the London press correctly evaluates British science, but the New York reviewers said what no psychologist would have said. American psychology is or is not behavioristic, entirely according to what is meant by the term. It is not Watsonian. There is no other leader. Yet there is almost no one left who would limit psychology to the results of introspection. If that be behaviorism, let England make the most of it.

There are a number of other passages which have evoked my dissent, but they concern minor matters. For one thing, McDougall's drainage theory seems to me not to be correct but unknown (as Flugel says, p. 270), but to have provoked wide unfavorable attention. I liked the theory and used it; it was Henry Head who derided me for so doing. Then, for another thing, Flugel repeats the old Nordic fallacy (p. 318) that persons of Germanic stock have more *g* than Southern Europeans. If this statement is based on the results of the U. S. Army tests, I can say only that the editors of that volume did not so interpret the research.

Finally there are the trivia. I have a list of fourteen items which I regard as errors of fact. They are not pernicious. They do not greatly affect the value of the book to psychologists. But it is customary to give them, and there ought to be this constant effort to correct the record in spite of the fallibility of all authors. Here they are:

Bell did not discuss the muscle sense in his pamphlet of 1811 but in an article of 1826, where he himself cites what Thomas Brown and others had said earlier (pp. 46 f.; cf. p. 27; Carmichael, *Psychol. Rev.*, 33, 1926, 204-209).

The parallelism between the histological structure of the brain and the connection

of ideas by association could hardly have been apparent to even the brightest student in 1833 (p. 55). It was not until 1842 that Helmholtz showed that the nerve-cells of ganglia are individually connected with fibers. Cajal's discovery of the nature of the synapse was in 1889, and Waldeyer's neuron theory in 1891, when associationistic reflexology became possible and ready for Dewey's objection to it in 1896.

Brentano was more of a protegé than a pupil of Lotze's (p. 87).

Flugel describes Helmholtz's measurement of the velocity of the nerve-impulse by the use of a kymograph, which he says Helmholtz invented (p. 89). It was the pendulum myograph with the smoked glass plate that Helmholtz invented and used in that experiment, and which was used so generally for a long time. All of Wundt's curves for muscle-twitches in his *Physiologische Psychologie* up to 1910 are myograph curves. Ludwig invented the kymograph a little later. One account (G. S. Hall) says that he got the idea from Helmholtz's myograph; another (*Britannica*) says that he adapted a kymograph invented by Thomas Young.

Bessel's relative personal equation did not really measure difference of speed in reaction (p. 90); the absolute personal equation (ca. 1854 et seq.) did.

I do not see how anyone can show that savages have a smaller two-point threshold than civilized persons (p. 95) unless he can get the savages carefully to control their criteria of judgment (cf. Boring, this JOURNAL, 32, 1921, 465-470).

Fechner's jest was that the moon (not man) is made of iodine (p. 97; printer's error?).

The date of Mach's *Analyse* is 1886 (p. 146), not 1885 as I unfortunately also misprinted it in the first printing of my *History*.

Jastrow's associate was C. S. Peirce (not Pierce), another error for which my *History* in its first printing may be responsible (p. 166).

Von Frey found that small stimuli, like aesthesiometer hairs, on the skin are approximately equivalent in intensity when pressure/radius is constant. This result was quite accurately established and is the ground for Von Frey's famous surface-tension theory of pressure. Flugel says that Von Frey's measure was pressure/area (p. 195), but that value holds only for large areal stimuli with rapid loading, and then not at all accurately.

It was the Catholic psychologist, T. V. Moore, once Külpe's student, who found that meanings sometimes arise more rapidly than images, not A. W. Moore, the Chicago philosopher, associated with Dewey and Angell in functionalism (p. 240).

The upper human threshold for hearing is about 20,000 cycles; the old value of 50,000 (p. 258) is long outmoded and was undoubtedly due to an artifact of the Galton whistle calibrated by the Kundt dust method.

Stanley Hall had Freud and Jung lecturing at Clark in 1909, not 1912 (p. 283).

Psychological Abstracts has never been published by the Clark University Press (p. 361).

Inevitably a review gets lost in criticisms, objections, and corrections. Let me repeat then that Flugel has written a readable, interesting and informative book, with a broad scope and a great amount of information, with several chapters that are as good brief accounts of their subjects as can be found anywhere, and with enough of a London bias to keep the Americans from becoming too complaisant.

E. G. B.

Modern Clinical Psychiatry. By ARTHUR P. NOYES. Philadelphia, W. B. Saunders Co., 1934. Pp. 485.

This book is a very readable exposition of psychiatry in its present status. The first four chapters are concerned with the psychological foundations of psychiatry. There are then chapters on each of the following topics: causes of mental disease, symptoms, "psychobiological constitution and reaction types," and the psychiatric examination. The remainder of the book describes the diagnostic categories of disorder. Although the order is shifted, the categories are those adopted by the Ameri-

can Psychiatric Association. No references are cited, but there is a short list of readings given at the end of each chapter.

Psychologists will be interested in this exposition of the psychological foundations of psychiatry, for, in the reviewer's opinion, it represents the psychology most prevalent among psychiatrists at the present time. Paraphrasing briefly: The organism is a unitary system. The organism as a whole has new qualities and modes of activity, *e.g.* striving, perceiving, and thinking. Mind is a collective designation for all those activities and phenomena that result when the organism functions as a whole. Behavior is the unity of the organism and the environment. All protoplasm has a psychic element; the higher psyche is derived from the lower in the phylogenetic scale. Speculatively, consciousness probably has its roots in the very irritability of protoplasm. Consciousness is a prerequisite for that degree of adapting to and making use of the environment to which is applied the term intelligence.

Upon this dual "unitary organism" are grafted "psychic energy," an unconscious, and the psychoanalytic mechanisms. "Psychic energy" is said to originate from three dynamic, biomental sources. (1) It arises from instincts which in psychological terms are more than inherited neurone circuits; they are specific forms of energy. (2) It arises from emotion which is (a) an agent that transmutes the potential energy of the organism into psychic energy, and (b) the subjective aspect of an obstructed instinct. (3) It arises from the "wish" which is a conscious or unconscious striving for release of inherent energy in the form of instinctive patterns. Personality is introduced as that organization of constitutional, affective, ideational, and conative capacities and tendencies which characterizes the individual. The mental mechanisms are devices by means of which the whole organism attempts to elaborate its purposes. Etiology is discussed as if all these standpoints and doctrines were one. Symptoms are classified under the psychological rubrics of the layman to which are added apperception and orientation.

Although his book contains nothing new, Dr. Noyes has performed a useful service in presenting this eclectic psychopathology. The book illustrates well the confusion which is current in the subject. A fair and readable statement, it should prove useful as a reference for classes in the psychology of the abnormal, serving at once as an introduction to the concepts of psychopathology and as a point of departure for developing in the student the attitude of friendly but rigorous criticism.

Worcester State Hospital JOSEPH MCV. HUNT
Worcester, Mass.

The Foundations of Psychology. By J. S. MOORE and HERBERT GURNEE. 2nd. ed., Princeton, Princeton University Press., 1933. Pp. xix, 287.

The present volume which brings the earlier work of Professor Moore down to date proposes to show (1) that scientific psychology is independent alike of metaphysics and of the biological sciences; (2) that introspection is the distinctive method of psychology; and (3) undertakes to establish some of the essential principles upon which such a complete and independent science is based.

The work is divided into three parts. Part I, which is concerned with the problem of definition, is devoted chiefly to a criticism of the various schools of psychology. In order to rescue psychology from the chaos of mutually conflicting "isms" and at the same time provide for the study of important but non-psychological problems the authors propose three relatively independent sciences; namely, psychology, the

science of mind; praxiology, the science of behavior; and psychopraxiology, a compound science concerned with the complex behavior of the psychophysiological organism in all its phases.

In Part II, which attempts to show that psychology is an independent science of the mental processes, the authors state that the privacy of mental experience constitutes the sole criterion by means of which the contents of psychology can be distinguished invariably from those of the material sciences. From this it follows that introspection, which is the only direct means for studying mental life, is the distinctive method of psychology. In order, however, to have a complete science of mind, introspection must be supplemented by three other methods, viz. observation of behavior, experimentation, and inference.

Part III is concerned with the postulates held to be necessary for the construction of a scientific psychology, with special reference to the problems of parallelism, psychical causation, and the subconscious. The interrelation between consciousness and the material world is twofold—psychophysical and psychophysiological. The latter type of interrelation gives rise to the well known principle of psychocerebral parallelism according to which every mental process is accompanied by some distinctive brain process, but in the description and explanation of the former only psychological concepts and terms may be employed. This "principle of independence" is emphasized throughout the discussion of psychical causation. According to Moore and Gurnee, if psychology is to be a complete and independent science, it must postulate that every mental phenomenon has a mental cause. All attempts to explain mental events in terms of brain processes are severely condemned—only confusion can result from mixing the two types of concepts in the attempt to explain either kind of event.

The authors' view with respect to the subconscious is the "logical" outcome of their acceptance of the principle of psychical independence. As a postulate, the subconscious is held to fulfill the demand for continuity in the mental life, and to afford the only satisfactory explanation of certain non-conscious mental phenomena.

Many of the opinions expressed in the book are admittedly open to dispute; in fact, the correctness of the authors' central position has already been denied by all those who regard psychology as an objective science. It seems to the reviewer that in attempting to maintain on the one hand the independence and self-sufficiency of psychology as the science of mind, and admitting on the other that introspection must be supplemented by the observation of behavior the authors are guilty of an inconsistency. If the study of behavior is not psychology (p. 55), then one wonders why a complete science of psychology must include the observation of behavior (p. 194). On the whole, however, the present volume constitutes a clear and concise exposition of the theoretical foundations of psychology as found in the different (divergent views are always presented the reader where the matter is controversial) "schools," and as such may be profitably read by advanced students in psychology.

Vanderbilt University EUGENE G. BUGG

The Mind and Its Body: The Foundations of Psychology. By CHARLES FOX. New York and London, Harcourt, Brace & Co., 1932. Pp. xii, 316.

Since, in the opinion of the author, psychology is both immediately and ultimately concerned with mind-body relationships, it is his purpose in this volume to offer a

sound philosophic basis upon which it shall rest. The problem of establishing an independent philosophic foundation is rendered especially difficult as a result of existing logical, psychological, and neurological presumptions. The need, however, is imperative; for contemporary research in such fields as physics, neurology, and physiology clearly reveals evidence challenging the metaphysical assumptions of the eighteenth century.

The discussion covers a considerable range of topics and a wealth of detail bearing upon the main problem of the relation of mind and body. The first four chapters of the book deal with the contributions of physiology and neurology to our understanding of the mind-body problem. Three dogmas associated with nerve physiology which most intimately touch upon the problem are: (1) the concept of the reflex as the sole type of nervous activity and that every kind of nervous action is composed of reflex units; (2) the belief in a one to one correspondence between brain events and mental events; (3) the belief that the cerebral cortex is the organ of consciousness, or is in some special sense connected with consciousness. The author concludes that nerve physiology, as represented by the work of Adrian and Lapicque, has nothing to offer in the solution of the really fundamental problems of psychology. Experimental evidence, particularly that of Lashley, on the effects of extirpation and the restitution of function following such procedure is interpreted as making totally untenable the physiological and neurological concepts of the mind-body relationship.

The topic of personality—including character and temperament—the author considers to lie outside of the field of scientific psychology. It is ruled out because the basic assumption which has guided research in this sphere has been that of a correspondence between physiological conditions and the temperament and character peculiarities of the individual. For the same reason the author refuses to subscribe to the concept of "fund of mental energy" employed by McDougall and posited by Spearman in their interpretations, respectively, of instinctive action and mental activity. Since the concept of instinct in connection with human activity ordinarily implies a special fund of energy, a special neural mechanism subserving a particular function, or perhaps both of these, it is altogether inapplicable to human activity because such activity does not depend upon any special mechanism. The evidence from experimental physiology upon this point is decisive and is further substantiated by the psychological experimentation of Lashley.

It is the opinion of the author that an adequate approach to an understanding of mind is to be made through a comprehension of the distinction between sensation and image. These are the ultimate data of psychology. The *sine qua non* of sensation is a living organism whose nerve impulses stimulate the mind which responds by an elaboration of consciousness. Images, however, consist of a revival of subjective states which formed a part of an original perception. They may thus be supposed to be independent of the brain centers of the corresponding sensations and the author believes that it is not necessary to assume any physiological basis for them whatsoever. This leads to a consideration of the nature of subjective activity in general and an analysis reveals a fundamental disparity between such activity and all forms of bodily activity. Subjective activity is general, whereas, physical activity is specific. The explanation offered is that subjective activity presupposes a spiritual world from which the motive force is derived so that what is impossible in the realm of nature is easily realized in the realm of ends.

Reviewing the opinions of those philosophers who have had anything original or constructive to say concerning the mind-body relationship, the author concludes that there is substantial evidence to show that no explanatory value whatever attaches to any of the concepts derived from the external world of science when these are applied to a study of the mind, that the only concepts which can intelligibly be used in describing the biological activities of living organisms are mental and that mental concepts by themselves are insufficient. He holds further that the problem of the relation of mind to body requires a further set of notions derived from the spiritual world, and suggests the ego, spiritual forces, the mind, and the living body. The living body is directed by mental factors whilst the mind is controlled by spiritual forces.

The author points out that the discussions presented in this volume are preliminary to a second volume in which he proposes to carry forward his arguments to demonstrate the immortality of the soul. It seems to the reviewer that the scientific psychologist will find little in this volume that transects his fields of interest and that the arguments and discussions may more properly merit the consideration of the philosopher and the logician. The author's interpretations and trenchant criticisms of the contributions which neurology and physiology have supposedly made to our understanding of the problems of human activity should arouse a wholesome skepticism against the "reasonableness" of many neurological and physiological dicta. The reader will find in the last two chapters of the book a comprehensive and critical discussion of the opinions of contemporary philosophers regarding the problem of the relationship of mind and matter.

There is appended a selected list of references for each chapter in the book. The volume is adequately indexed.

University of Kentucky G. B. DIMMICK

Phantastica: Narcotic and Stimulating Drugs, Their Use and Abuse. By LOUIS LEWIN. Tr. from 2nd German ed. by. P. W. A. Wirth. New York, E. P. Dutton & Co., 1931. Pp. xi, 335.

This extraordinary volume (a slimmer version of which appeared first in 1925) is the work of a German pharmacologist and toxicologist, who takes credit for first bringing to the notice of science, in 1886, both the Mexican peyotl or mescal buttons (*Anhalonium Lewinii*), and the South Sea Island 'kava-kava,' a drink prepared from *Piper Methysticum*. Earlier publications of the author are freely cited, including *Die Nebenwirkungen der Arzneimitteln: Die Gifte in der Weltgeschichte*, and *Die Wirkungen von Arzneimitteln und Giften auf das Auge.*

Catering obviously to the popular zest for marvels, *Phantastica* is hardly a scientific treatise. Masses of oddly assorted and not uninteresting detail—historical, geographic, botanical, anecdotal—make up its content; of value primarily, however, to the ethnologist and traveller rather than the psychologist. The latter will turn to it in vain for authenticated accounts of the physiological and psychological effects of the vast array of drugs listed (mainly habit-forming), ranging from morphia to the less known *kat* and fly agaric. Uncritical rehearsal of anecdotes and striking instances of misuse is too often the line of procedure. Experiments on animals and human subjects are occasionally cited, but withal sceptically. For not only is the psychology of the writer naïve, he is afflicted with a curious pessimism as to the

fitness of physiological and psychological methods to the work in hand. Owing to the personal equation, and individual variation in the susceptibility to the various agents, he puts his trust rather in his own 'observations' than in scientific experiment. Yet the instances he presents are vitiated by precisely the type of defect he ascribes to science—inability or failure to report the precise status of the individual prior to experiment or addiction, especially whether normal or psychotic.

The caliber of the author's psychology and physiology is exposed further in his classification of the drugs "capable of effecting a modification of the cerebral functions." The five groups run as follows (1) Euphorica, sedatives of mental activity, diminishing the functions of emotion and perception, inducing a state of physical and mental well-being (morphine, codeine, etc.). (2) Phantastica, hallucinating substances, the drugs of illusion (peyotl, hashish, scapalomine, the loco herbs). (3) Inebriantia, evoking a primary phase of cerebral excitation, followed by depression (alcohol, chloroform, ether, etc.). (4) Hypnotica, soporifics, sleep-producing agents, such as chloral, veronal. (5) Excitantia, mental stimulants (caffeine, tobacco, betel, maté). Obviously, the groups are overlapping, the salient effect depending on the size of the dose, habituation, etc.

The world-wide use of all five classes—betel and hashish (*Cannabis indica*) are declared to number hundreds of millions among their addicts—inspires the pharmacologist to flights of eloquence. Though his avowed aim is the bringing to public notice of the growing menace of addiction, he hails the use of these secret gifts of Nature by primitive peoples as first evidence of an evolving aesthetic and religious sense, elevating man above humdrum material needs. Repeatedly, he waxes mystical and rhapsodic over the beneficent 'instinct' which leads ignorant races (Hindoo, Indian, Koryak, Kaffir) to single out of the vegetable kingdom the precise species possessing the property of alleviating the monotony of existence, its drudgery and hardships.

Interspersed with passages of this nature, and citations from the poets, are graphic accounts of organic and mental deterioration following excessive use of all and sundry narcotics. Though sententiously conceding the insignificance of the ruin of thousands as compared with the vast sweep of 'cosmic process' (an echo, perhaps, from Spengler), the writer endorses educational campaigns to combat excesses the effects of which may ultimately impinge on the lives and interests of all of us—prohibition and edicts having proven in all ages unavailing.

For all its defects, its lapses into mysticism and obscurantism, the book performs a certain office, affording the psychologist a fresh, if disturbing, insight into the aids with which the work of the world is accomplished, and its wars waged; a new perspective on the bases of the 'cultures,' the religious and social trends, peculiar to various primitive and semi-primitive groups. The translation is usually, not invariably, satisfactory; and there is a fairly full index. The precise chemical designation of the active principle of a number of the drugs listed is given.

Cornell University ELSIE MURRAY

Creative Mind. By C. SPEARMAN. New York, D. Appleton & Co., 1931. Pp. xii, 162. Sixteen pl. and 6 fig.

This slight volume, which one can run through in little more than an hour, is essentially an expansion of a chapter and a rather flippant paragraph in the author's

earlier and more ponderous *Nature of Intelligence* (1923), and *The Abilities of Man* (1927). Stemming avowedly from Aristotle and the Schoolmen, the discoverer of *g*, promulgator of the only 'scientific' present-day psychology, is a stout adherent of mental powers and faculties, deprecating sensory images, in the fashion of Bühler, as "mere satellites of thought." All artistic and scientific creation or invention, the whole of aesthetics, adaptive behavior, strategy, statesmanship, and hallucination to boot are accordingly stowed neatly under the "noegenetic" rubric, "eduction of correlates."

Actually, this gets us little further than Grant Allen's *Physiological Aesthetics*, and Sully's handling of dreams and imagination. In its analysis of experience into 'fundaments,' its associationism, its plodding utilitarianism, the book is typically in the older British tradition. The newer expressionistic trends of Bullough's and Valentine's work are ignored. True, the familiar facts and principles of aesthetics —unity, variety, repetition, etc.—are clearly, sometimes even eloquently set forth, with Ruskinian sentiment; only to be cleverly pigeonholed under the Spearman trinity—apprehension, eduction of relations, eduction of correlates (the latter in point of fact only our old friend association by similarity, with a little *vis a tergo* added). The book might serve as an extension course primer (actually it is the first volume in Aveling's Library of Contemporary Psychology, and more than once does obeisance to the editor-in-chief); but though it numbers among its illustrations a Pissarro and a Braque, it is too Mid-Victorian in tone, too academic, too didactic, to convey to the younger generation any notion of the subtleties of modern art—or for that matter of classic (note the blundering comments on Giorgone—p. 60). The grip and thrill of genuine aesthetic experience are apparently beyond the author. At best he views an art-work as a lure to the teasing out of the axioms and laws he has imposed upon psychology *as science*.

The whole treatment, the effort to lay bare the 'springs and mechanism' of creative process suffers from extreme intellectualism and oversimplification. Essentially the excursion of a statistician into realities (though lacking the felicity of the forays of a Dodgson or an Eddington), it ignores the directive and selective potency of moods and emotions. Though cleverly disposing of the myth of the 'subconscious' in inspiration, Spearman fails to exploit fruitfully the real basis of this mistaken notion. Even on the purely intellectual side, his measuring-worm notion of 'eduction' is inadequate to the complexities of creative process and the shifting of 'constellations' of ideation, the truly organic growth involved. All creative products, on the lines of his interpretation, are Frankensteinian monsters; their occasional galvanizing into life remains a mystery. If his 'noegenetic' formula were indeed the key to creation, it is matter for amazement that its holders (even allowing for variations in the level of *g*) are not more frequently discovering missing elements, or hidden galaxies; devising cleverer scenarios, or forging better deals.

Cornell University ELSIE MURRAY

The World of the Blind: A Psychological Study. By PIERRE VILLEY. Tr. by Alys Hallard. New York, Macmillan Co., 1930. Pp. 403. One plate.

This little volume, which was awarded honors by the French Academy of Moral and Political Sciences, and which carries a laudatory foreword by Janet, is the work of a man blind from early childhood, who nevertheless has achieved rank on the literary faculty of the University of Caen.

The writer's declared purpose is to show, on the basis of his own experience, and that of the many men and women of his acquaintance, similarly afflicted, that the blind (Diderot to the contrary) live in an integrated spatial world, similar to that of the seeing; and, with proper aids and instruction, are capable of meeting the latter on their own level.

Part I, The Intelligence of the Blind, recounts the history of their liberation during the last century and a quarter, through adapted instruction and the invention of Braille. The 77 characters of the latter are shown in the frontispiece; and the author's use of it in preparing his thesis for the Sorbonne is described in detail. Part II, under the caption 'Substitution of the Senses,' maintains, in harmony with Griesbach's experimental findings, that the psychological achievements of the blind are based not on lowered tactual and auditory thresholds, but on the acquisition of more complex significations by the sensations of touch. Crogius and Kunz's reduction of the 'sense of obstacles' to warmth and pressure (or smell) Villey rejects in favor of auditory clues (Truschel's view) though admitting synaesthesial frontal sensations accompanying the latter. A discussion of orientation in the blind, and of their ability in gymnastics and in manual occupations of varying degrees of complexity, follows, with an abundance of concrete illustrations.

Part III, perhaps of greatest interest to the psychologist, under the heading "Substitution of Images and the Furniture of the Mind," treats of the integration of successive tactual and muscle sensations into spatial representations, in the case both of sizable objects, and of specific localities, as a room or a village. Citing evidence pro and con, the author maintains that the spatial perception of the blind is as synthetic and unified as that of the seeing. A geographical map, however, in all its complexities of detail, is, he concedes, beyond the reach of clear conception, where the visual function is excluded.

Part IV, "Indications with regard to the Affective Life," deals with the impression of personality of the blind, and their enjoyment of nature, art, and poetry. While distrusting the verbalizations of Helen Keller as to the appreciation of sculpture, especially where facial expression and posture are concerned, Villey maintains that auditory, olfactory, tactual, and muscular imagery are adequate to the full interpretation and enjoyment of poetry, and the various esthetic aspects of nature. Part V takes up the practical problem of the handicap of the blind in society.

The book is written in an agreeable, flowing style with frequent reference to the experiments and observations of others. It furnishes us with unique testimony on the part of a critical and analytical member of this sector of the under endowed; along with proof that, given effort and a certain mental endowment, the handicap of sense deprivation may be overcome. There is a detailed, 7-page table of contents, but no index. One could wish that the author's proof-readers had done better by the work of this gifted blind scholar. The ludicrous and misleading caption, Substitution of Lenses, stands uncorrected on one of the early pages of the volume.

Cornell University ELSIE MURRAY

Psychology: An Empirical Study of Behavior. By FREDERICK H. LUND. New York, Ronald Press, 1933. Pp. xv, 475.

The present volume presents psychology from the empirical and biological point of view in such a manner as to reduce considerably the hiatus which frequently exists in elementary texts between the discussion of behavior mechanisms and that of behavior.

The first part of the book, which is devoted chiefly to those constitutional and structural features which are basic to behavior, profits by recent neurological and physiological studies. Unfortunately, in his discussion of auditory perception the author apparently confuses the consonance value of a musical interval with its relative pleasingness, stating that the intervals which are most pleasing and consonant are those represented by the simpler ratios. Furthermore, the notion that intervals which are normally considered dissonant, may with practice come to seem consonant, is not sufficiently established to be put forward without qualification.

After the discussion of behavior mechanisms, emotional reactions, and motivation, attention is directed chiefly toward the principles of modification. Two chapters are devoted to the learning process. In the treatment of this topic the author leans rather heavily upon the principle of redintegration which was advocated (in a somewhat different form) by Hamilton and James Mill about a century ago. This principle is held to be basic to all learning and to all acquisition, whether it be of the type belonging to the early modifications of infant behavior, or of the type familiar in the building up of complex mental, verbal, and perceptual functions. Thus, notwithstanding that the author criticizes traditional logic on the score of over-simplification of the thought processes, his redintegrative principle would, if so widely applied, tend to do for psychology about what the syllogism is alleged to have done for logic. The author appropriates this principle rather skillfully to some of the problems of behavior, but it may well be doubted whether he is justified in reducing so many complex forms of response to any one simple formula, especially when the latter is only partially understood.

The discussion of learning is followed by a short chapter dealing with social and verbal behavior. In his treatment of perception the author sets forth clearly and concisely the relationship which obtains between sensory and perceptual processes. In fact, from the pedagogical point of view the author's discussion of this topic compares favorably with that found in any elementary text which has come to the reviewer's attention.

Lund's treatment of thinking is typically "objective" and as such shares both the advantages and the disadvantages which usually attach to this approach to the problem. For him, thinking is a form of doing—of implicit or substitute doing, differing from the explicit only in the extent to which the adjustments are overtly manifest. That a naturalistic and dynamic view of mental activity is preferable to the old tautological and somewhat mystical conceptions cannot reasonably be doubted. The author's account of thinking, however, strikes the reviewer as an over-simplification of the matter, and at times one has the impression that mere verbalisms are offered as explanations. For example, in one place (p. 373) thinking is referred to as a "mental rehearsal," while in another (p. 375) it is "merely a less complete form of acting." As a matter of methodology there is no objection to proceeding upon the assumption that every mental event has its physical counterpart, but in the light of the present state of knowledge concerning the nature of the nerve impulse and of cerebral activity it is not at all certain that the only distinction between thinking and overt activity is that of magnitude of movement.

The discussion of the topics sleeping, dreaming, imagining, individual differences, intelligence, which constitutes the last three chapters, meets satisfactorily the demands of an elementary text. The reviewer had the impression, however, that the

discussion dealing with the correlation of abilities would have profited by reference to Spearman's study of this problem.

On the whole the present volume is well written, and effects a satisfactory balance between experimental studies and systematic doctrine. Each chapter contains a list of questions and exercises, and one of references. S. W. Ranson is misspelled "Ransom."

Vanderbilt University EUGENE G. BUGG

Keeping a Sound Mind. By JOHN J. B. MORGAN. New York, Macmillan Co., 1934. Pp. ix, 440.

The purpose of this book is to present the principles of mental health so simply and untechnically that college freshmen who have had no training in psychology may read it understandingly. The author has done this very effectively. The style is personal and didactical, the book being common-sense advice about such practical problems as one's mental health, what to fight for and how to fight, overcoming mental conflicts, adjusting one's emotional difficulties, compensating for defects, getting things done, getting along with people and developing self-confidence. There is also a brief discussion of the causes of crime.

There is no bibliography and there are few citations in the text to original studies. The books suggested for further reading are similar in style and purpose to the text. The questions at the end of each chapter provide for a detailed test of the reader's memory of the facts presented. Since the treatment is so simplified questions to stimulate thinking about and observation of the types of behavior discussed would seem to be more effective.

The experiment demonstrating the possibility of eliminating an acquired fear of a rabbit by the method of unconditioning is inaccurately attributed to J. B. Watson instead of to Mrs. Mary Cover Jones (p. 75).

Such admonitions in regard to dealing successfully with people as "let the other person feel superior to you" "learn to listen and show an interest in others," "to give indirect praise and to make it easy for people to say yes to you" are probably good advice, but to what extent these principles have been proved by an accurate analysis of those personality traits effective in social adjustment is not shown. Much research is needed to verify these views even though they may seem to be logically true.

The book is, however, one of the best of its kind and should be of value to beginning college students. The statement regarding an objective evaluation of one's mental health and the discussion of "What to fight for" and "How to fight" are the best chapters in the book.

Georgia State College for Women EURI BELLE BOLTON

A New Physiological Psychology. By W. BURRIDGE. Foreword by Leonard Hill. London, Edward Arnold & Co., 1933. Pp. vii, 158.

This book, as its title suggests, is a departure from the traditional concepts of physiological psychology. It aims to explain the functional characteristics of sense organs and nervous system in terms of certain conceptions which have arisen out of the author's extensive studies on the heart. The new doctrines are summarized as follows: "(1) that central neurones and end-organs are rhythmically active structures whose possessions in excitability determine their intrinsic activity only; (2) that

rhythmically active structures possess a quality, responsiveness, a capacity to have their activities altered by environmental change, which so differs from excitability that the rhythmical structure with much excitability has little responsiveness; (3) that excitable tissues possess two sources of potential energy, the kinesiphores, whose interactions provide the dynamic energy for evoking responses; (4) that the proportions between the two interacting kinesiphores are capable of infinite variation; (5) that in the organ of mind consciousness is mediated through the one kinesiphore and the data of a sensation or idea through the other; (6) that every organ of mind has its own individual limit of effort derived from the two kinesiphores in such manner that the more of the one type of energy used the less can there be used of the other."

Needless to say the experimental findings of studies of nerve-muscle preparations are either summarily dismissed as not applying to rhythmically acting structures or are re-interpreted in the light of the new postulates. Moreover, the author seems not to be acquainted with most current physiological psychology, and his interpretations of the points of view of such psychology may be seriously questioned. It must be noted also that the experimental facts resulting from studies of the heart form an extremely weak foundation for the large superstructure of explanation which is builded upon it. The author has been markedly influenced by the Freudian doctrine and in many ways the concepts, faculties, and entities which he creates to account for behavior savor strongly of the mysticism of this psychology.

Most of the chapters of the book consist of applications of the new theories to familiar neurological and psychological topics.

University of California C. W. BROWN

Psychology. By SHEPHERD IVORY FRANZ and KATE GORDON. New York, McGraw-Hill, 1933. Pp. xvii, 494.

Psychology Work Book. By SHEPHERD IVORY FRANZ and KATE GORDON. New York, McGraw-Hill, 1932. Pp. ix, 227.

Psychology is intended for the elementary course and results from the collaboration of ten authors: Mason N. Crook, Frank C. Davis, Evelene F. Davis, S. Carolyn Fisher, Shepherd Ivory Franz, Lawrence Gahagan, Frances I. Gaw, J. A. Gengerelli, H. C. Gilhousen, and Kate Gordon.

Considering its multiple authorship, the book is surprisingly unified as to viewpoint, style, and content. Probably the outstanding characteristic of the book is its tendency to emphasize neural bases of behavior, and even to attempt neural schematizations to account for various defects in behavior. So strikingly is this illustrative of the viewpoint of the late Dr. Franz that one acquainted with his life's work in the rehabilitation of unfortunates may read certain sections as though Dr. Franz had written, or were speaking, in the first person (especially true in Chapter VII, "Nervous Structures and Functions"). This insistence upon an intimate relationship between mind and body is accompanied by the presentation of new data and theory and by a serious and unbiased attempt to evaluate them. Unfortunately, references for supplementary reading and to psychological literature are almost totally lacking in *Psychology* though they are abundant and conveniently placed in the *Work Book.*

In the opinion of the reviewer, the language of the text is too difficult—cer-

tainly too pedantic—for the general run of college and university sophomores. Chapter II draws too widely from the field of literature, approaching the anecdotal rather than the experimental in far too many instances. The reviewer is impressed that an attempt to approach psychology philosophically, and to justify rather than describe, has obscured the presentation of psychological fact and methodology in certain instances. This is especially apparent in the discussion of "How Psychology developed" (p. 3).

From the standpoint of usefulness, the *Work Book* (by Franz and Gordon alone) appears to be superior to the text. It is clear, concise, calls for very little laboratory equipment, gives the student practically all the tools and paper with which to work, and, as previously stated, has an abundance of citations to the original literature. The anecdotal note persists to some extent, however. One wonders why the student is referred to the writings of Henry Ford, Helen Keller, and Booker T. Washington for biographical data when materials such as those found in the three volumes of "Genetic Studies of Genius" are available.

Louisville, Kentucky MILTON B. JENSEN

Problems of Neurosis. By ALFRED ADLER. Edited by Philippe Mairet. New York, Cosmopolitan Book Corp., 1930. Pp. v, 244.

In this treatise, Adler applies the principles of his individual psychology to the behavior of neurotics. The theme is developed by means of brief citations of illustrative phases of case histories, with extensive interpretations. There is much repetition of a few cardinal ideas, especially that of a goal of superiority (and consequent compensation for inferiority) as the guiding principle for the explanation of human behavior. From this point of view, the manner in which the individual meets the three great problems of occupational, social, and sexual adjustment is evolved. Stress is laid upon the importance of the first four or five years as establishing the prototype for the patient's 'style of life,' and upon the rôles played by the members of the immediate family. Sex is everywhere secondary to the goal of superiority. The reader cannot but be impressed by Adler's cleverness in fitting all the case material into his formulations. The simplicity of the explanations of conduct hardly makes up for the lack of any logical or genetic evidence of their validity, so that one is left with the feeling that too much depends upon the author's intuitive genius in understanding human motivation. The book is prefaced with a philosophical note by F. G. Crookshank which is highly laudatory of Adler and pictures him as the leader of a totally new idea in medicine as well as in psychiatry.

Mental Hygiene Clinic GILBERT J. RICH
Milwaukee, Wis.

Seeing: A Partnership of Lighting and Vision. By M. LUCKIESH and FRANK K. MOSS. Baltimore, Williams & Wilkins Co., 1931. Pp. viii, 241; suppl. 26. Sixty fig., 47 tabl., 26 pl.

While essentially an argument for light and more of it, this volume from Nela Park contains much of value to, and frequently ignored by, the average psychologist —not to mention the lighting experts responsible for academic mal-illumination. The experimentalist will find valuable data in Ch. IX (The Pupil of the Eye), precise definition of various units in Ch. VI (Brightness), and in Ch. XIII (Spectral Charac-

ter of Light) a discussion of the daylight equivalents of artificial sources. Applied psychology also can reap a rich harvest in these pages. Here are tables bearing on the relation of acuity to illumination; and kindred matters, including points on the aesthetics of color. One notes a few dubious statements, *i.e.* the limitation of the visible spectrum to a range of 4000 to 7600 Ångstroms (p. 140); and the assertion (p. 144) that a "blue light cannot be focussed accurately by most persons except at very close range," surely a case of myopic vision.

Various acuity tests are reproduced, including the broken circle, parallel lines, and an original "demonstration visual test." In the latter (reported in this JOURNAL, 45, 1933, 135 ff.), broken diagonals (the breaks forming letters), offer distracting incentives to eye movement, and are utilized to determine optimal illumination for the efficient functioning of the eye under industrial conditions.

Cornell University ELSIE MURRAY

Kleine Einführung in die Charakterkunde. By H. ROHRACHER. Leipzig, Tuebner, 1934. Pp. 138.

The German theories about the origin of character variations among individuals are very neatly described in this little volume. The author favors the biological theory of Kretschmer, but gives much space also to the somewhat metaphysical theorizing of Klages. Other German authors, treated in less detail, are Jaensch, Jung, Ewald, Spranger. The style is commendably simple and clear; and the book will be a very helpful introduction into the German terminology for the student who is in need of it. Whether Kretschmer's division into two types (the character of the fat and the character of the lean) is a scientific achievement, time will tell. The same can be said about the division of Jaensch into the character of the "eidetic" and that of the weakly visualizing person, with numerous subdivisions of these types. The typology of Klages, based on distinguishing *Seele and Geist,* or devotion and assertion, as we might translate into English, is less likely to appeal to the American psychologist. Spranger distinguishes six types, the theorist, the economist, the estheticist, the socialist, the Machtmensch (could we translate "Hitlerist"?) and the religionist.

University of Missouri MAX F. MEYER

The Concentric Method in the Diagnosis of Psychoneurotics. By M. LAIGNEL-LAVASTINE. New York, Harcourt, Brace & Co., 1931. Pp. xii, 217.

The book comprises 10 informal clinical lectures which the author delivered in 1927. Six of the lectures are directly related to the diagnosis of psychoneurotics; the other four (on emotivity, the unconscious self of psychoneurotics, the devil, and general principles in the treatment of psychoneurotics) were intended to complete and amplify the six basic lectures. The concentric method of diagnosis is not a "superficial" method. It requires a study of each of five zones in a psychoneurosis: "the peripheral psychic atmosphere, the nervous zone, the endocrine zone, the visceral zone, and the central morbific kernel." Experts in the field of the abnormal are probably already familiar with the work of Laignel-Lavastine; amateurs will hardly find it worth their while to read this informal description of his methods and principles.

Clark University E. H. KEMP

The Rhetoric of Aristotle: An Expanded Translation with Supplementary Examples for Students of Composition and Public Speaking. By LANE COOPER. New York, D. Appleton & Co., 1932. Pp. xlviii, 259.

The *Rhetoric* should be familiar to all serious students of psychology but especially to those students of social psychology who are engaged upon clear descriptions of psychological congregates. The work is very rich in many varieties of the 'audience' congregate, *i.e.* in the speaker-audient relations. The careful psychologist will know how to distinguish the arts of valuation, in which Aristotle's works abound, from sober description. Professor Cooper's translation, his scholarly and appreciative analyses, and his textual annotations greatly increase the usefulness of the text. The graduate student and the young instructor in psychology who set their teeth into this great work will learn much about their own foundations and about the limitations of their subject. M. B.

Introductory Acoustics. By GEORGE WALTER STEWART. New York, D. Van Nostrand Co., 1933 (?). Pp. xi, 200.

The physicist is accustomed to deal with non-living systems. As a rule, he is not quite at home among biological and psychological facts and principles. Professor Stewart has researched enough with psychologists to have a sympathetic regard for the activities of the living human body. While those chapters of his book which touch upon hearing (as distinguished from sound-energies) stand closer to acoustical engineering than to psychological description, they will be useful by way of general orientation to our younger students of psychology, who should read the book in connection with their Helmholtz, Stumpf, and other classics, and as an aid in the current periodical literature, which has grown technical and difficult. M. B.

Adam et Eve, ou la question des sexes. By W. BOVEN. Neuchatel and Paris, Delachaux & Niestlé, 1933. Pp. 144.

Besides the usual descriptions, warnings, and hygienic prescriptions, the well-written little book emphasizes the important differences between sex, "qui est l'instrument de l'amour," and gender, "qui est tout le reste." It is refreshing to find accorded to socialized gender much that has commonly been set down as sexual. M. B.

The Bundle of Life. By JANE REVERE BURKE. New York, E. P. Dutton & Co., 1934. Pp. xiv, 174.

This book is the report of a spiritualist who has been receiving messages from William James. A reader who is familiar with James' writings might wonder why his famous literary style does not appear in his messages from the spirit world. The book might possibly afford some material for students of abnormal psychology.

Ball State Teachers College, Muncie, Indiana. R. C. SCARF

Meaning and the Western Way. By CHANDLER BENNITT. New York, Hidden Press, 1933. Pp. viii, 245.

This book is a general discussion of meaning and actuality. It purports to lean upon psychology, particularly upon that of Jung. Its contact with psychological data and with psychological theory is slight.

University of Missouri JOHN A. MCGEOCH

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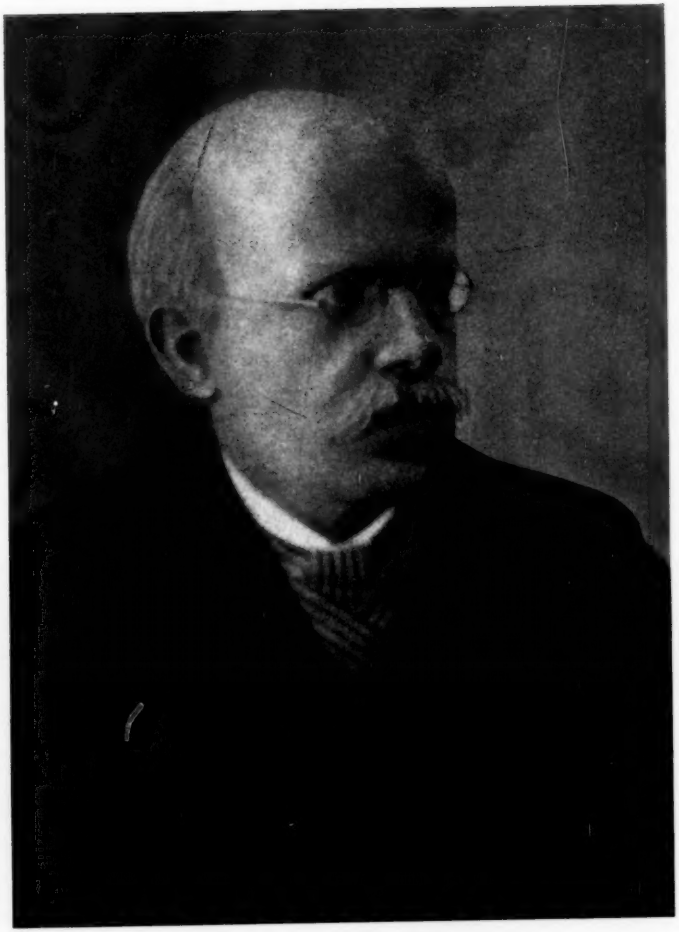

G. E. Müller

(*See* page 344)